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In This Issue

Joan W. Scott, in reviewing the use of "gender" by feminist historians, argues that Marxism, psychoanalysis, and certain feminist theories of the origins of patriarchy have limited explanatory power because of their reductive causality or their focus on the fixed meaning of the binary opposition of male and female. Scott proposes a "deconstruction" of that binary opposition through an analysis of the historical construction of gender. She sees in history a persistent relationship between the signification of gender and the signification of power. Changes in gender relationships accompany changes in the representation of power. The study of gender signification therefore provides new avenues not only into the history of the relationship between men and women but also into the concepts of politics and power that shape all forms of social organization.

Another method of symbolizing power is described in an essay by **Anne Walthall** on the social construction of the deeds of martyred peasant leaders from Japan's Tokugawa period (1600–1868). The lessons built on the lives of these "righteous men" (*gimin*) have served causes that span the political spectrum, but the common role of these symbols has been to provide a focus for collective identity among subordinated social groups. Despite the special power of these symbols in Japanese culture, Walthall believes that their operation corresponds to that of other types of historical representation that subordinated social groups elsewhere have drawn on to form and maintain a political and social identity.

During the anniversary year of the United States Constitution, we hope to be able to present a number of articles that illuminate the history of the Constitution. We begin with an essay by **Martyn P. Thompson** that offers a new history of the concept of fundamental law in Western political discourse up to the American Revolution. Previous histories have linked the concept narrowly to a theory of constitutional law as the customary law of ancient constitutions and have confined discussion of the term to French, English, and American traditions. Thompson argues that, prior to the American Revolution, fundamental law was connected to European contractualist thought and to the architectural imagery of the state in the foundation-edifice metaphor. The term "fundamental law" acquired the meaning of constitutional law through a process of interpretation that extended over a long period and included important contributions from thinkers in central Europe as well as from French and English writers.

The question of continuity and change in the demographic behavior of immigrants is taken up by **John W. Briggs**. He avoids some of the pitfalls inherent in broad comparisons of national groups by reconstructing the marriage and childbirth cultures of emigrants who moved from a single Italian village to an American city. He discovers that change did not occur in a straight line. Some behaviors changed, others remained the same, and, in the case of premarital conceptions, the immigrants were better able to live up to traditional norms than were the villagers who remained in Italy. Briggs stresses the importance of his method in providing the type of evidence that can sustain comparative analyses of the cultural effects of emigration.

Seconding this argument for tightly controlled investigations is **Eric H. Monkkonen**, who questions recent appeals to the profession for more synthesis. Those who complain about narrowness and fragmentation in the field of history fail to understand that increasing technical sophistication is a concomitant of the growth of professional quality. In assessing the two fields in which he has worked, Monkkonen contends that early efforts at synthesis in U.S. urban history may have stunted its research agenda, while the virtual absence of synthesis in criminal justice history has allowed that field to flourish and to build a foundation of empirical research that can support enduring generalizations.

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Gender: A Useful Category of Historical Analysis

JOAN W. SCOTT

> Gender. n. a grammatical term only. To talk of persons or creatures of the masculine or feminine gender, meaning of the male or female sex, is either a jocularity (permissible or not according to context) or a blunder.
>
> (Fowler's *Dictionary of Modern English Usage*, Oxford, 1940).

THOSE WHO WOULD CODIFY THE MEANINGS OF WORDS fight a losing battle, for words, like the ideas and things they are meant to signify, have a history. Neither Oxford dons nor the Académie Française have been entirely able to stem the tide, to capture and fix meanings free of the play of human invention and imagination. Mary Wortley Montagu added bite to her witty denunciation "of the fair sex" ("my only consolation for being of that gender has been the assurance of never being married to any one among them") by deliberately misusing the grammatical reference.[1] Through the ages, people have made figurative allusions by employing grammatical terms to evoke traits of character or sexuality. For example, the usage offered by the *Dictionnaire de la langue française* in 1876 was, "On ne sait de quel genre il est, s'il est mâle ou femelle, se dit d'un homme très-caché, dont on ne connait pas les sentiments."[2] And Gladstone made this distinction in 1878: "Athene has nothing of sex except the gender, nothing of the woman except the form."[3] Most recently—too recently to find its way into dictionaries or the *Encyclopedia of the Social Sciences*—feminists have in a more literal and serious vein begun to use "gender" as a way of referring to the social organization of the relationship between the sexes. The connection to grammar is both explicit and full of unexamined possibilities. Explicit because the grammatical usage involves formal

This article is for Elizabeth Weed, who taught me how to think about gender and theory. It was first prepared for delivery at the meeting of the American Historical Association in New York City, December 27, 1985. I am deeply grateful to Denise Riley, who showed me how a historian might work with and through theory; also to Janice Doane, Jasmine Ergas, Anne Norton, and Harriet Whitehead, all members of the seminar on "Cultural Constructions of Gender" held at Brown University's Pembroke Center for Teaching and Research on Women during 1982–85. Suggestions and criticisms from members of the Historical Studies Workshop at the New School for Social Research, especially Ira Katznelson, Charles Tilly, and Louise A. Tilly, forced me to clarify the argument in important ways. Comments from other friends and colleagues have also been extremely helpful, especially those of Elisabetta Galeotti, Rayna Rapp, Christine Stansell, and Joan Vincent. Donald Scott, as always, was at once my most demanding and supportive critic.

[1] *Oxford English Dictionary* (1961 edn.), vol. 4.

[2] E. Littré, *Dictionnaire de la langue française* (Paris, 1876).

[3] Raymond Williams, *Keywords* (New York, 1983), 285.

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rules that follow from the masculine or feminine designation; full of unexamined possibilities because in many Indo-European languages there is a third category—unsexed or neuter.

In its most recent usage, "gender" seems to have first appeared among American feminists who wanted to insist on the fundamentally social quality of distinctions based on sex. The word denoted a rejection of the biological determinism implicit in the use of such terms as "sex" or "sexual difference." "Gender" also stressed the relational aspect of normative definitions of femininity. Those who worried that women's studies scholarship focused too narrowly and separately on women used the term "gender" to introduce a relational notion into our analytic vocabulary. According to this view, women and men were defined in terms of one another, and no understanding of either could be achieved by entirely separate study. Thus Natalie Davis suggested in 1975, "It seems to me that we should be interested in the history of both women and men, that we should not be working only on the subjected sex any more than an historian of class can focus entirely on peasants. Our goal is to understand the significance of the *sexes*, of gender groups in the historical past. Our goal is to discover the range in sex roles and in sexual symbolism in different societies and periods, to find out what meaning they had and how they functioned to maintain the social order or to promote its change."[4]

In addition, and perhaps most important, "gender" was a term offered by those who claimed that women's scholarship would fundamentally transform disciplinary paradigms. Feminist scholars pointed out early on that the study of women would not only add new subject matter but would also force a critical reexamination of the premises and standards of existing scholarly work. "We are learning," wrote three feminist historians, "that the writing of women into history necessarily involves redefining and enlarging traditional notions of historical significance, to encompass personal, subjective experience as well as public and political activities. It is not too much to suggest that however hesitant the actual beginnings, such a methodology implies not only a new history of women, but also a new history."[5] The way in which this new history would both include and account for women's experience rested on the extent to which gender could be developed as a category of analysis. Here the analogies to class (and race) were explicit; indeed, the most politically inclusive of scholars of women's studies regularly invoked all three categories as crucial to the writing of a new history.[6] An interest in class, race, and gender signaled first, a scholar's commitment to a history that included stories of the oppressed and an analysis of the meaning and nature of their oppression and, second, scholarly understanding that inequalities of power are organized along at least three axes.

[4] Natalie Zemon Davis, "Women's History in Transition: The European Case," *Feminist Studies*, 3 (Winter 1975–76): 90.

[5] Ann D. Gordon, Mari Jo Buhle, and Nancy Shrom Dye, "The Problem of Women's History," in Berenice Carroll, ed., *Liberating Women's History* (Urbana, Ill., 1976), 89.

[6] The best and most subtle example is from Joan Kelly, "The Doubled Vision of Feminist Theory," in her *Women, History and Theory* (Chicago, 1984), 51–64, especially 61.

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The litany of class, race, and gender suggests a parity for each term, but, in fact, that is not at all the case. While "class" most often rests on Marx's elaborate (and since elaborated) theory of economic determination and historical change, "race" and "gender" carry no such associations. No unanimity exists among those who employ concepts of class. Some scholars employ Weberian notions, others use class as a temporary heuristic device. Still, when we invoke class, we are working with or against a set of definitions that, in the case of Marxism, involve an idea of economic causality and a vision of the path along which history has moved dialectically. There is no such clarity or coherence for either race or gender. In the case of gender, the usage has involved a range of theoretical positions as well as simple descriptive references to the relationships between the sexes.

Feminist historians, trained as most historians are to be more comfortable with description than theory, have nonetheless increasingly looked for usable theoretical formulations. They have done so for at least two reasons. First, the proliferation of case studies in women's history seems to call for some synthesizing perspective that can explain continuities and discontinuities and account for persisting inequalities as well as radically different social experiences. Second, the discrepancy between the high quality of recent work in women's history and its continuing marginal status in the field as a whole (as measured by textbooks, syllabi, and monographic work) points up the limits of descriptive approaches that do not address dominant disciplinary concepts, or at least that do not address these concepts in terms that can shake their power and perhaps transform them. It has not been enough for historians of women to prove either that women had a history or that women participated in the major political upheavals of Western civilization. In the case of women's history, the response of most non-feminist historians has been acknowledgment and then separation or dismissal ("women had a history separate from men's, therefore let feminists do women's history, which need not concern us"; or "women's history is about sex and the family and should be done separately from political and economic history"). In the case of women's participation, the response has been minimal interest at best ("my understanding of the French Revolution is not changed by knowing that women participated in it"). The challenge posed by these responses is, in the end, a theoretical one. It requires analysis not only of the relationship between male and female experience in the past but also of the connection between past history and current historical practice. How does gender work in human social relationships? How does gender give meaning to the organization and perception of historical knowledge? The answers depend on gender as an analytic category.

For the most part, the attempts of historians to theorize about gender have remained within traditional social scientific frameworks, using longstanding formulations that provide universal causal explanations. These theories have been limited at best because they tend to contain reductive or overly simple generalizations that undercut not only history's disciplinary sense of the complexity of social causation but also feminist commitments to analyses that will lead to change.

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A review of these theories will expose their limits and make it possible to propose an alternative approach.[7]

THE APPROACHES USED BY MOST HISTORIANS fall into two distinct categories. The first is essentially descriptive; that is, it refers to the existence of phenomena or realities without interpreting, explaining, or attributing causality. The second usage is causal; it theorizes about the nature of phenomena or realities, seeking an understanding of how and why these take the form they do.

In its simplest recent usage, "gender" is a synonym for "women." Any number of books and articles whose subject is women's history have, in the past few years, substituted "gender" for "women" in their titles. In some cases, this usage, though vaguely referring to certain analytic concepts, is actually about the political acceptability of the field. In these instances, the use of "gender" is meant to denote the scholarly seriousness of a work, for "gender" has a more neutral and objective sound than does "women." "Gender" seems to fit within the scientific terminology of social science and thus dissociates itself from the (supposedly strident) politics of feminism. In this usage, "gender" does not carry with it a necessary statement about inequality or power nor does it name the aggrieved (and hitherto invisible) party. Whereas the term "women's history" proclaims its politics by asserting (contrary to customary practice) that women are valid historical subjects, "gender" includes but does not name women and so seems to pose no critical threat. This use of "gender" is one facet of what might be called the quest of feminist scholarship for academic legitimacy in the 1980s.

But only one facet. "Gender" as a substitute for "women" is also used to suggest that information about women is necessarily information about men, that one implies the study of the other. This usage insists that the world of women is part of the world of men, created in and by it. This usage rejects the interpretive utility of the idea of separate spheres, maintaining that to study women in isolation perpetuates the fiction that one sphere, the experience of one sex, has little or nothing to do with the other. In addition, gender is also used to designate social relations between the sexes. Its use explicitly rejects biological explanations, such as those that find a common denominator for diverse forms of female subordination in the facts that women have the capacity to give birth and men have greater muscular strength. Instead, gender becomes a way of denoting "cultural constructions"—the entirely social creation of ideas about appropriate roles for women and men. It is a way of referring to the exclusively social origins of the subjective identities of men and women. Gender is, in this definition, a social category imposed on a sexed body.[8] Gender seems to have become a particularly useful word as studies of sex and sexuality have proliferated, for it offers a way of differentiating sexual practice from the social roles assigned to women and men.

[7] For a review of recent work on women's history, see Joan W. Scott, "Women's History: The Modern Period," *Past and Present*, 101 (1983): 141–57.

[8] For an argument against the use of gender to emphasize the social aspect of sexual difference, see Moira Gatens, "A Critique of the Sex/Gender Distinction," in J. Allen and P. Patton, eds., *Beyond Marxism? Interventions after Marx* (Sydney, 1983), 143–60.

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Although scholars acknowledge the connection between sex and (what the sociologists of the family called) "sex roles," these scholars do not assume a simple or direct linkage. The use of gender emphasizes an entire system of relationships that may include sex, but is not directly determined by sex or directly determining of sexuality.

These descriptive usages of gender have been employed by historians most often to map out a new terrain. As social historians turned to new objects of study, gender was relevant for such topics as women, children, families, and gender ideologies. This usage of gender, in other words, refers only to those areas—both structural and ideological—involving relations between the sexes. Because, on the face of it, war, diplomacy, and high politics have not been explicitly about those relationships, gender seems not to apply and so continues to be irrelevant to the thinking of historians concerned with issues of politics and power. The effect is to endorse a certain functionalist view ultimately rooted in biology and to perpetuate the idea of separate spheres (sex or politics, family or nation, women or men) in the writing of history. Although gender in this usage asserts that relationships between the sexes are social, it says nothing about why these relationships are constructed as they are, how they work, or how they change. In its descriptive usage, then, gender is a concept associated with the study of things related to women. Gender is a new topic, a new department of historical investigation, but it does not have the analytic power to address (and change) existing historical paradigms.

Some historians were, of course, aware of this problem, hence the efforts to employ theories that might explain the concept of gender and account for historical change. Indeed, the challenge was to reconcile theory, which was framed in general or universal terms, and history, which was committed to the study of contextual specificity and fundamental change. The result has been extremely eclectic: partial borrowings that vitiate the analytic power of a particular theory or, worse, employ its precepts without awareness of their implications; or accounts of change that, because they embed universal theories, only illustrate unchanging themes; or wonderfully imaginative studies in which theory is nonetheless so hidden that these studies cannot serve as models for other investigations. Because the theories on which historians have drawn are often not spelled out in all their implications, it seems worthwhile to spend some time doing that. Only through such an exercise can we evaluate the usefulness of these theories and, perhaps, articulate a more powerful theoretical approach.

Feminist historians have employed a variety of approaches to the analysis of gender, but they come down to a choice between three theoretical positions.[9] The first, an entirely feminist effort, attempts to explain the origins of patriarchy. The second locates itself within a Marxian tradition and seeks there an accommodation with feminist critiques. The third, fundamentally divided between French post-structuralist and Anglo-American object-relations theorists, draws on these

[9] For a somewhat different approach to feminist analysis, see Linda J. Nicholson, *Gender and History: The Limits of Social Theory in the Age of the Family* (New York, 1986).

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different schools of psychoanalysis to explain the production and reproduction of the subject's gendered identity.

Theorists of patriarchy have directed their attention to the subordination of women and found their explanation for it in the male "need" to dominate the female. In Mary O'Brien's ingenious adaptation of Hegel, she defined male domination as the effect of men's desire to transcend their alienation from the means of the reproduction of the species. The principle of generational continuity restores the primacy of paternity and obscures the real labor and the social reality of women's work in childbirth. The source of women's liberation lies in "an adequate understanding of the process of reproduction," an appreciation of the contradiction between the nature of women's reproductive labor and (male) ideological mystifications of it.[10] For Shulamith Firestone, reproduction was also the "bitter trap" for women. In her more materialist analysis, however, liberation would come with transformations in reproductive technology, which might in some not too distant future eliminate the need for women's bodies as the agents of species reproduction.[11]

If reproduction was the key to patriarchy for some, sexuality itself was the answer for others. Catherine MacKinnon's bold formulations were at once her own and characteristic of a certain approach: "Sexuality is to feminism what work is to marxism: that which is most one's own, yet most taken away." "Sexual objectification is the primary process of the subjection of women. It unites act with word, construction with expression, perception with enforcement, myth with reality. Man fucks woman; subject verb object."[12] Continuing her analogy to Marx, MacKinnon offered, in the place of dialectical materialism, consciousness-raising as feminism's method of analysis. By expressing the shared experience of objectification, she argued, women come to understand their common identity and so are moved to political action. For MacKinnon, sexuality thus stood outside ideology, discoverable as an unmediated, experienced fact. Although sexual relations are defined in MacKinnon's analysis as social, there is nothing except the inherent inequality of the sexual relation itself to explain why the system of power operates as it does. The source of unequal relations between the sexes is, in the end, unequal relations between the sexes. Although the inequality of which sexuality is the source is said to be embodied in a "whole system of social relationships," how this system works is not explained.[13]

Theorists of patriarchy have addressed the inequality of males and females in important ways, but, for historians, their theories pose problems. First, while they offer an analysis internal to the gender system itself, they also assert the primacy of that system in all social organization. But theories of patriarchy do not show how gender inequality structures all other inequalities or, indeed, *how* gender affects

[10] Mary O'Brien, *The Politics of Reproduction* (London, 1981), 8–15, 46.
[11] Shulamith Firestone, *The Dialectic of Sex* (New York, 1970). The phrase "bitter trap" is O'Brien's, *Politics of Reproduction*, 8.
[12] Catherine McKinnon, "Feminism, Marxism, Method, and the State: An Agenda for Theory," *Signs*, 7 (Spring 1982): 515, 541.
[13] *Ibid.*, 541, 543.

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those areas of life that do not seem to be connected to it. Second, whether domination comes in the form of the male appropriation of the female's reproductive labor or in the sexual objectification of women by men, the analysis rests on physical difference. Any physical difference takes on a universal and unchanging aspect, even if theorists of patriarchy take into account the existence of changing forms and systems of gender inequality.[14] A theory that rests on the single variable of physical difference poses problems for historians: it assumes a consistent or inherent meaning for the human body—outside social or cultural construction—and thus the ahistoricity of gender itself. History becomes, in a sense, epiphenomenal, providing endless variations on the unchanging theme of a fixed gender inequality.

Marxist feminists have a more historical approach, guided as they are by a theory of history. But, whatever the variations and adaptations have been, the self-imposed requirement that there be a "material" explanation for gender has limited or at least slowed the development of new lines of analysis. Whether a so-called dual-systems solution is proffered (one that posits the separate but interacting realms of capitalism and patriarchy) or an analysis based more firmly in orthodox Marxist discussions of modes of production is developed, the explanation for the origins of and changes in gender systems is found outside the sexual division of labor. Families, households, and sexuality are all, finally, products of changing modes of production. That is how Engels concluded his explorations of the *Origins of the Family*;[15] that is where economist Heidi Hartmann's analysis ultimately rests. Hartmann insisted on the importance of taking into account patriarchy and capitalism as separate but interacting systems. Yet, as her argument unfolds, economic causality takes precedence, and patriarchy always develops and changes as a function of relations of production. When she suggested that "it is necessary to eradicate the sexual division of labor itself to end male domination," she meant ending job segregation by sex.[16]

Early discussions among Marxist feminists circled around the same set of problems: a rejection of the essentialism of those who would argue that the "exigencies of biological reproduction" determine the sexual division of labor under capitalism; the futility of inserting "modes of reproduction" into discussions of modes of production (it remains an oppositional category and does not assume equal status with modes of production); the recognition that economic systems do not directly determine gender relationships, indeed, that the subordination of women pre-dates capitalism and continues under socialism; the search nonetheless

[14] For an interesting discussion of the strengths and limits of the term "patriarchy," see the exchange between historians Sheila Rowbotham, Sally Alexander, and Barbara Taylor in Raphael Samuel, ed., *People's History and Socialist Theory* (London, 1981), 363–73.

[15] Frederick Engels, *The Origins of the Family, Private Property, and the State* (1884; reprint edn., New York, 1972).

[16] Heidi Hartmann, "Capitalism, Patriarchy, and Job Segregation by Sex," *Signs*, 1 (Spring 1976): 168. See also "The Unhappy Marriage of Marxism and Feminism: Towards a More Progressive Union," *Capital and Class*, 8 (Summer 1979): 1–33; "The Family as the Locus of Gender, Class, and Political Struggle: The Example of Housework," *Signs*, 6 (Spring 1981): 366–94.

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for a materialist explanation that excludes natural physical differences.[17] An important attempt to break out of this circle of problems came from Joan Kelly in her essay, "The Doubled Vision of Feminist Theory," where she argued that economic and gender systems interacted to produce social and historical experiences; that neither system was causal, but both "operate simultaneously to reproduce the socioeconomic and male-dominant structures of [a] particular social order." Kelly's suggestion that gender systems had an independent existence provided a crucial conceptual opening, but her commitment to remain within a Marxist framework led her to emphasize the causal role of economic factors even in the determination of the gender system: "The relation of the sexes operates in accordance with, and through, socioeconomic structures, as well as sex/gender ones."[18] Kelly introduced the idea of a "sexually based social reality," but she tended to emphasize the social rather than the sexual nature of that reality, and, most often, "social," in her usage, was conceived in terms of economic relations of production.

The most far-reaching exploration of sexuality by American Marxist feminists is in *Powers of Desire*, a volume of essays published in 1983.[19] Influenced by increasing attention to sexuality among political activists and scholars, by French philosopher Michel Foucault's insistence that sexuality is produced in historical contexts, and by the conviction that the current "sexual revolution" required serious analysis, the authors made "sexual politics" the focus of their inquiry. In so doing, they opened the question of causality and offered a variety of solutions to it; indeed, the real excitement of this volume is its lack of analytic unanimity, its sense of analytic tension. If individual authors tend to stress the causality of social (by which is often meant "economic") contexts, they nonetheless include suggestions about the importance of studying "the psychic structuring of gender identity." If "gender ideology" is sometimes said to "reflect" economic and social structures, there is also a crucial recognition of the need to understand the complex "link between society and enduring psychic structure."[20] On the one hand, the editors endorse Jessica Benjamin's point that politics must include attention to "the erotic, fantastic components of human life," but, on the other, no essays besides Benjamin's deal fully or seriously with the theoretical issues she raises.[21] Instead,

[17] Discussions of Marxist feminism include Zillah Eisenstein, *Capitalist Patriarchy and the Case for Socialist Feminism* (New York, 1979); A. Kuhn, "Structures of Patriarchy and Capital in the Family," in A. Kuhn and A. Wolpe, eds., *Feminism and Materialism* (London, 1978); Rosalind Coward, *Patriarchal Precedents* (London, 1983); Hilda Scott, *Does Socialism Liberate Women?* (Boston, 1974); Jane Humphries, "Working Class Family, Women's Liberation and Class Struggle: The Case of Nineteenth-Century British History," *Review of Radical Political Economics*, 9 (1977): 25–41; Jane Humphries, "Class Struggle and the Persistence of the Working Class Family," *Cambridge Journal of Economics*, 1 (1971): 241–58; and see the debate on Humphries's work in *Review of Radical Political Economics*, 12 (Summer 1980): 76–94.

[18] Kelly, "Doubled Vision of Feminist Theory," 61.

[19] Ann Snitow, Christine Stansell, and Sharon Thompson, eds., *Powers of Desire: The Politics of Sexuality* (New York, 1983).

[20] Ellen Ross and Rayna Rapp, "Sex and Society: A Research Note from Social History and Anthropology," in *Powers of Desire*, 53.

[21] "Introduction," *Powers of Desire*, 12; and Jessica Benjamin, "Master and Slave: The Fantasy of Erotic Domination," *Powers of Desire*, 297.

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a tacit assumption runs through the volume that Marxism can be expanded to include discussions of ideology, culture, and psychology and that this expansion will happen through the kind of concrete examination of evidence undertaken in most of the articles. The advantage of such an approach lies in its avoidance of sharp differences of position, the disadvantage in its leaving in place an already fully articulated theory that leads back from relations of the sexes based to relations of production.

A comparison of American Marxist-feminist efforts, exploratory and relatively wide-ranging, to those of their English counterparts, tied more closely to the politics of a strong and viable Marxist tradition, reveals that the English have had greater difficulty in challenging the constraints of strictly determinist explanations. This difficulty can be seen most dramatically in the recent debates in the *New Left Review* between Michèle Barrett and her critics, who charged her with abandoning a materialist analysis of the sexual division of labor under capitalism.[22] It can be seen as well in the replacement of an initial feminist attempt to reconcile psychoanalysis and Marxism with a choice of one or another of these theoretical positions by scholars who earlier insisted that some fusion of the two was possible.[23] The difficulty for both English and American feminists working within Marxism is apparent in the works I have mentioned here. The problem they face is the opposite of the one posed by patriarchal theory. Within Marxism, the concept of gender has long been treated as the by-product of changing economic structures; gender has had no independent analytic status of its own.

A REVIEW OF PSYCHOANALYTIC THEORY REQUIRES a specification of schools, since the various approaches have tended to be classified by the national origins of the founders and the majority of the practitioners. There is the Anglo-American school, working within the terms of theories of object-relations. In the U.S., Nancy Chodorow is the name most readily associated with this approach. In addition, the work of Carol Gilligan has had a far-reaching impact on American scholarship,

[22] Johanna Brenner and Maria Ramas, "Rethinking Women's Oppression," *New Left Review*, 144 (March–April 1984): 33–71; Michèle Barrett, "Rethinking Women's Oppression: A Reply to Brenner and Ramas," *New Left Review*, 146 (July–August 1984): 123–28; Angela Weir and Elizabeth Wilson, "The British Women's Movement," *New Left Review*, 148 (November–December 1984): 74–103; Michèle Barrett, "A Response to Weir and Wilson," *New Left Review*, 150 (March–April 1985): 143–47; Jane Lewis, "The Debate on Sex and Class," *New Left Review*, 149 (January–February 1985): 108–20. See also Hugh Armstrong and Pat Armstrong, "Beyond Sexless Class and Classless Sex: Towards Feminist Marxism," *Studies in Political Economy*, 10 (Winter 1983): 7–44; Hugh Armstrong and Pat Armstrong, "Comments: More on Marxist Feminism," *Studies in Political Economy*, 15 (Fall 1984): 179–84; and Jane Jenson, "Gender and Reproduction; or, Babies and the State," unpublished paper, June 1985, pp. 1–7.

[23] For early theoretical formulations, see *Papers on Patriarchy: Conference, London 76* (London, 1976). I am grateful to Jane Caplan for telling me of the existence of this publication and for her willingness to share with me her copy and her ideas about it. For the psychoanalytic position, see Sally Alexander, "Women, Class and Sexual Difference," *History Workshop*, 17 (Spring 1984): 125–35. In seminars at Princeton University in early 1986, Juliet Mitchell seemed to be returning to an emphasis on the priority of materialist analyses of gender. For an attempt to get beyond the theoretical impasse of Marxist feminism, see Coward, *Patriarchal Precedents*. See also the brilliant American effort in this direction by anthropologist Gayle Rubin, "The Traffic in Women: Notes on the 'Political Economy' of Sex," in Rayna R. Reiter, ed., *Towards an Anthropology of Women* (New York, 1975): 167–68.

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including history. Gilligan's work draws on Chodorow's, although it is concerned less with the construction of the subject than with moral development and behavior. In contrast to the Anglo-American school, the French school is based on structuralist and post-structuralist readings of Freud in terms of theories of language (for feminists, the key figure is Jacques Lacan).

Both schools are concerned with the processes by which the subject's identity is created; both focus on the early stages of child development for clues to the formation of gender identity. Object-relations theorists stress the influence of actual experience (the child sees, hears, relates to those who care for it, particularly, of course, to its parents), while the post-structuralists emphasize the centrality of language in communicating, interpreting, and representing gender. (By "language," post-structuralists do not mean words but systems of meaning—symbolic orders—that precede the actual mastery of speech, reading, and writing.) Another difference between the two schools of thought focuses on the unconscious, which for Chodorow is ultimately subject to conscious understanding and for Lacan is not. For Lacanians, the unconscious is a critical factor in the construction of the subject; it is the location, moreover, of sexual division and, for that reason, of continuing instability for the gendered subject.

In recent years, feminist historians have been drawn to these theories either because they serve to endorse specific findings with general observations or because they seem to offer an important theoretical formulation about gender. Increasingly, those historians working with a concept of "women's culture" cite Chodorow's or Gilligan's work as both proof of and explanation for their interpretations; those wrestling with feminist theory look to Lacan. In the end, neither of these theories seems to me entirely workable for historians; a closer look at each may help explain why.

My reservation about object-relations theory concerns its literalism, its reliance on relatively small structures of interaction to produce gender identity and to generate change. Both the family division of labor and the actual assignment of tasks to each parent play a crucial role in Chodorow's theory. The outcome of prevailing Western systems is a clear division between male and female: "The basic feminine sense of self is connected to the world, the basic masculine sense of self is separate."[24] According to Chodorow, if fathers were more involved in parenting and present more often in domestic situations, the outcome of the oedipal drama might be different.[25]

[24] Nancy Chodorow, *The Reproduction of Mothering: Psychoanalysis and the Sociology of Gender* (Berkeley, Calif., 1978), 169.

[25] "My account suggests that these gender-related issues may be influenced during the period of the oedipus complex, but they are not its only focus or outcome. The negotiation of these issues occurs in the context of broader object-relational and ego processes. These broader processes have equal influence on psychic structure formation, and psychic life and relational modes in men and women. They account for differing modes of identification and orientation to heterosexual objects, for the more asymmetrical oedipal issues psychoanalysts describe. These outcomes, like more traditional oedipal outcomes, arise from the asymmetrical organization of parenting, with the mother's role as primary parent and the father's typically greater remoteness and his investment in socialization especially in areas concerned with gender-typing." Chodorow, *Reproduction of Mothering*, 166. It is important to note that there are differences in interpretation and approach between Chodorow and British object-

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This interpretation limits the concept of gender to family and household experience and, for the historian, leaves no way to connect the concept (or the individual) to other social systems of economy, politics, or power. Of course, it is implicit that social arrangements requiring fathers to work and mothers to perform most child-rearing tasks structure family organization. Where such arrangements come from and why they are articulated in terms of a sexual division of labor is not clear. Neither is the issue of inequality, as opposed to that of asymmetry, addressed. How can we account within this theory for persistent associations of masculinity with power, for the higher value placed on manhood than on womanhood, for the way children seem to learn these associations and evaluations even when they live outside nuclear households or in households where parenting is equally divided between husband and wife? I do not think we can without some attention to symbolic systems, that is, to the ways societies represent gender, use it to articulate the rules of social relationships, or construct the meaning of experience. Without meaning, there is no experience; without processes of signification, there is no meaning (which is not to say that language is everything, but a theory that does not take it into account misses the powerful roles that symbols, metaphors, and concepts play in the definition of human personality and human history.)

Language is the center of Lacanian theory; it is the key to the child's induction into the symbolic order. Through language, gendered identity is constructed. According to Lacan, the phallus is the central signifier of sexual difference. But the meaning of the phallus must be read metaphorically. For the child, the oedipal drama sets forth the terms of cultural interaction, since the threat of castration embodies the power, the rules of (the father's) law. The child's relationship to the law depends on sexual difference, on its imaginative (or fantastic) identification with masculinity or femininity. The imposition, in other words, of the rules of social interaction are inherently and specifically gendered, for the female necessarily has a different relationship to the phallus than the male does. But, gender identification, although it always appears coherent and fixed, is, in fact, highly unstable. Like words themselves, subjective identities are processes of differentiation and distinction, requiring the suppression of ambiguities and opposite elements in order to assure (and create the illusion of) coherence and common understanding. The idea of masculinity rests on the necessary repression of feminine aspects—of the subject's potential for bisexuality—and introduces conflict into the opposition of masculine and feminine. Repressed desires are present in the unconscious and are constantly a threat to the stability of gender identification, denying its unity, subverting its need for security. In addition, conscious ideas of masculine or feminine are not fixed, since they vary according

relations theorists who follow the work of D. W. Winicott and Melanie Klein. Chodorow's approach is best characterized as a more sociological or sociologized theory, but it is the dominant lens through which object-relations theory has been viewed by American feminists. On the history of British object-relations theory in relation to social policy, see Denise Riley, *War in the Nursery* (London, 1984).

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to contextual usage. Conflict always exists, then, between the subject's need for the appearance of wholeness and the imprecision of terminology, its relative meaning, its dependence on repression.[26] This kind of interpretation makes the categories of "man" and "woman" problematic by suggesting that masculine and feminine are not inherent characteristics but subjective (or fictional) constructs. This interpretation also implies that the subject is in a constant process of construction, and it offers a systematic way of interpreting conscious and unconscious desire by pointing to language as the appropriate place for analysis. As such, I find it instructive.

I am troubled, nonetheless, by the exclusive fixation on questions of "the subject" and by the tendency to reify subjectively originating antagonism between males and females as the central fact of gender. In addition, although there is openness in the concept of how "the subject" is constructed, the theory tends to universalize the categories and relationship of male and female. The outcome for historians is a reductive reading of evidence from the past. Even though this theory takes social relationships into account by linking castration to prohibition and law, it does not permit the introduction of a notion of historical specificity and variability. The phallus is the only signifier; the process of constructing the gendered subject is, in the end, predictable because always the same. If, as film theorist Teresa de Lauretis suggests, we need to think in terms of the construction of subjectivity in social and historical contexts, there is no way to specify those contexts within the terms offered by Lacan. Indeed, even in de Lauretis's attempt, social reality (that is, "material, economic and interpersonal [relations] which are in fact social, and in a larger perspective historical") seems to lie outside, apart from the subject.[27] A way to conceive of "social reality" in terms of gender is lacking.

The problem of sexual antagonism in this theory has two aspects. First, it projects a certain timeless quality, even when it is historicized as well as it has been by Sally Alexander. Alexander's reading of Lacan led her to conclude that "antagonism between the sexes is an unavoidable aspect of the acquisition of sexual identity . . . If antagonism is always latent, it is possible that history offers no final resolution, only the constant reshaping, reorganizing of the symbolization of difference, and the sexual division of labor."[28] It may be my hopeless utopianism that gives me pause before this formulation, or it may be that I have not yet shed the episteme of what Foucault called the Classical Age. Whatever the explanation, Alexander's formulation contributes to the fixing of the binary opposition of male and female as the only possible relationship and as a permanent aspect of the human condition. It perpetuates rather than questions what Denise Riley refers to as "the dreadful air of constancy of sexual polarity." She writes: "The historically

[26] Juliet Mitchell and Jacqueline Rose, eds., *Jacques Lacan and the Ecole Freudienne* (London, 1983); Alexander, "Women, Class and Sexual Difference."
[27] Teresa de Lauretis, *Alice Doesn't: Feminism, Semiotics, Cinema* (Bloomington, Ind., 1984), 159.
[28] Alexander, "Women, Class and Sexual Difference," 135.

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constructed nature of the opposition [between male and female] produces as one of its effects just that air of an invariant and monotonous men/women opposition."[29]

It is precisely that opposition, in all its tedium and monotony, that (to return to the Anglo-American side) Carol Gilligan's work has promoted. Gilligan explained the divergent paths of moral development followed by boys and girls in terms of differences of "experience" (lived reality). It is not surprising that historians of women have picked up her ideas and used them to explain the "different voices" their work has enabled them to hear. The problems with these borrowings are manifold, and they are logically connected.[30] The first is a slippage that often happens in the attribution of causality: the argument moves from a statement such as "women's experience leads them to make moral choices contingent on contexts and relationships" to "women think and choose this way because they are women." Implied in this line of reasoning is the ahistorical, if not essentialist, notion of woman. Gilligan and others have extrapolated her description, based on a small sample of late twentieth-century American schoolchildren, into a statement about all women. This extrapolation is evident especially, but not exclusively, in the discussions by some historians of "women's culture" that take evidence from early saints to modern militant labor activists and reduce it to proof of Gilligan's hypothesis about a universal female preference for relatedness.[31] This use of Gilligan's ideas provides sharp contrast to the more complicated and historicized conceptions of "women's culture" evident in the *Feminist Studies* 1980 symposium.[32] Indeed, a comparison of that set of articles with Gilligan's formulations reveals the extent to which her notion is ahistorical, defining woman/man as a universal, self-reproducing binary opposition—fixed always in the same way. By insisting on fixed differences (in Gilligan's case, by simplifying data with more mixed results about sex and moral reasoning to underscore sexual difference), feminists contribute to the kind of thinking they want to oppose. Although they insist on the revaluation of the category "female" (Gilligan suggests that women's moral choices may be more humane than men's), they do not examine the binary opposition itself.

We need a refusal of the fixed and permanent quality of the binary opposition, a genuine historicization and deconstruction of the terms of sexual difference. We must become more self-conscious about distinguishing between our analytic vocabulary and the material we want to analyze. We must find ways (however imperfect) to continually subject our categories to criticism, our analyses to self-criticism. If we employ Jacques Derrida's definition of deconstruction, this

[29] Denise Riley, "Summary of Preamble to Interwar Feminist History Work," unpublished paper, presented to the Pembroke Center Seminar, May 1985, p. 11.

[30] Carol Gilligan, *In a Different Voice: Psychological Theory and Women's Development* (Cambridge, Mass., 1982).

[31] Useful critiques of Gilligan's book are: J. Auerbach, *et al.*, "Commentary on Gilligan's *In a Different Voice*," *Feminist Studies*, 11 (Spring 1985); and "Women and Morality," a special issue of *Social Research*, 50 (Autumn 1983). My comments on the tendency of historians to cite Gilligan come from reading unpublished manuscripts and grant proposals, and it seems unfair to cite those here. I have kept track of the references for over five years, and they are many and increasing.

[32] *Feminist Studies*, 6 (Spring 1980): 26–64.

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criticism means analyzing in context the way any binary opposition operates, reversing and displacing its hierarchical construction, rather than accepting it as real or self-evident or in the nature of things.[33] In a sense, of course, feminists have been doing this for years. The history of feminist thought is a history of the refusal of the hierarchical construction of the relationship between male and female in its specific contexts and an attempt to reverse or displace its operations. Feminist historians are now in a position to theorize their practice and to develop gender as an analytic category.

CONCERN WITH GENDER AS AN ANALYTIC CATEGORY has emerged only in the late twentieth century. It is absent from the major bodies of social theory articulated from the eighteenth to the early twentieth centuries. To be sure, some of those theories built their logic on analogies to the opposition of male and female, others acknowledged a "woman question," still others addressed the formation of subjective sexual identity, but gender as a way of talking about systems of social or sexual relations did not appear. This neglect may in part explain the difficulty that contemporary feminists have had incorporating the term gender into existing bodies of theory and convincing adherents of one or another theoretical school that gender belongs in their vocabulary. The term gender is part of the attempt by contemporary feminists to stake claim to a certain definitional ground, to insist on the inadequacy of existing bodies of theory for explaining persistent inequalities between women and men. It seems to me significant that the use of the word gender has emerged at a moment of great epistemological turmoil that takes the form, in some cases, of a shift from scientific to literary paradigms among social scientists (from an emphasis on cause to one on meaning, blurring genres of inquiry, in anthropologist Clifford Geertz's phrase),[34] and, in other cases, the form of debates about theory between those who assert the transparency of facts and those who insist that all reality is construed or constructed, between those who defend and those who question the idea that "man" is the rational master of his own destiny. In the space opened by this debate and on the side of the critique of science developed by the humanities, and of empiricism and humanism by post-structuralists, feminists have not only begun to find a theoretical voice of their own but have found scholarly and political allies as well. It is within this space that we must articulate gender as an analytic category.

What should be done by historians who, after all, have seen their discipline dismissed by some recent theorists as a relic of humanist thought? I do not think we should quit the archives or abandon the study of the past, but we do have to change some of the ways we have gone about working, some of the questions we

[33] By "deconstruction," I mean to evoke Derrida's discussion, which, though it surely did not invent the procedure of analysis it describes, has the virtue of theorizing it so that it can constitute a useful method. For a succinct and accessible discussion of Derrida, see Jonathan Culler, *On Deconstruction: Theory and Criticism after Structuralism* (Ithaca, N.Y., 1982), especially 156–79. See also Jacques Derrida, *Of Grammatology* (Baltimore, 1976); Jacques Derrida, *Spurs* (Chicago, 1979); and a transcription of Pembroke Center Seminar, 1983, in *Subjects/Objects* (Fall 1984).

[34] Clifford Geertz, "Blurred Genres," *American Scholar*, 49 (October 1980): 165–79.

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have asked. We need to scrutinize our methods of analysis, clarify our operative assumptions, and explain how we think change occurs. Instead of a search for single origins, we have to conceive of processes so interconnected that they cannot be disentangled. Of course, we identify problems to study, and these constitute beginnings or points of entry into complex processes. But it is the processes we must continually keep in mind. We must ask more often how things happened in order to find out why they happened; in anthropologist Michelle Rosaldo's formulation, we must pursue not universal, general causality but meaningful explanation: "It now appears to me that woman's place in human social life is not in any direct sense a product of the things she does, but of the meaning her activities acquire through concrete social interaction."[35] To pursue meaning, we need to deal with the individual subject as well as social organization and to articulate the nature of their interrelationships, for both are crucial to understanding how gender works, how change occurs. Finally, we need to replace the notion that social power is unified, coherent, and centralized with something like Foucault's concept of power as dispersed constellations of unequal relationships, discursively constituted in social "fields of force."[36] Within these processes and structures, there is room for a concept of human agency as the attempt (at least partially rational) to construct an identity, a life, a set of relationships, a society with certain limits and with language—conceptual language that at once sets boundaries and contains the possibility for negation, resistance, reinterpretation, the play of metaphoric invention and imagination.

My definition of gender has two parts and several subsets. They are interrelated but must be analytically distinct. The core of the definition rests on an integral connection between two propositions: gender is a constitutive element of social relationships based on perceived differences between the sexes, and gender is a primary way of signifying relationships of power. Changes in the organization of social relationships always correspond to changes in representations of power, but the direction of change is not necessarily one way. As a constitutive element of social relationships based on perceived differences between the sexes, gender involves four interrelated elements: first, culturally available symbols that evoke multiple (and often contradictory) representations—Eve and Mary as symbols of woman, for example, in the Western Christian tradition—but also, myths of light and dark, purification and pollution, innocence and corruption. For historians, the interesting questions are, which symbolic representations are invoked, how, and in what contexts? Second, normative concepts that set forth interpretations of the meanings of the symbols, that attempt to limit and contain their metaphoric possibilities. These concepts are expressed in religious, educational, scientific, legal, and political doctrines and typically take the form of a fixed binary opposition, categorically and unequivocally asserting the meaning of male and female, masculine and feminine. In fact, these normative statements depend on

[35] Michelle Zimbalist Rosaldo, "The Uses and Abuses of Anthropology: Reflections on Feminism and Cross-Cultural Understanding," *Signs*, 5 (Spring 1980): 400.

[36] Michel Foucault, *The History of Sexuality*, vol. 1, *An Introduction* (New York, 1980); Michel Foucault, *Power/Knowledge: Selected Interviews and Other Writings, 1972–77* (New York, 1980).

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the refusal or repression of alternative possibilities, and, sometimes, overt contests about them take place (at what moments and under what circumstances ought to be a concern of historians). The position that emerges as dominant, however, is stated as the only possible one. Subsequent history is written as if these normative positions were the product of social consensus rather than of conflict. An example of this kind of history is the treatment of the Victorian ideology of domesticity as if it were created whole and only afterwards reacted to instead of being the constant subject of great differences of opinion. Another kind of example comes from contemporary fundamentalist religious groups that have forcibly linked their practice to a restoration of women's supposedly more authentic "traditional" role, when, in fact, there is little historical precedent for the unquestioned performance of such a role. The point of new historical investigation is to disrupt the notion of fixity, to discover the nature of the debate or repression that leads to the appearance of timeless permanence in binary gender representation. This kind of analysis must include a notion of politics as well as reference to social institutions and organizations—the third aspect of gender relationships.

Some scholars, notably anthropologists, have restricted the use of gender to the kinship system (focusing on household and family as the basis for social organization). We need a broader view that includes not only kinship but also (especially for complex, modern societies) the labor market (a sex-segregated labor market is a part of the process of gender construction), education (all-male, single-sex, or coeducational institutions are part of the same process), and the polity (universal male suffrage is part of the process of gender construction). It makes little sense to force these institutions back to functional utility in the kinship system, or to argue that contemporary relationships between men and women are artifacts of older kinship systems based on the exchange of women.[37] Gender is constructed through kinship, but not exclusively; it is constructed as well in the economy and the polity, which, in our society at least, now operate largely independently of kinship.

The fourth aspect of gender is subjective identity. I agree with anthropologist Gayle Rubin's formulation that psychoanalysis offers an important theory about the reproduction of gender, a description of the "transformation of the biological sexuality of individuals as they are enculturated."[38] But the universal claim of psychoanalysis gives me pause. Even though Lacanian theory may be helpful for thinking about the construction of gendered identity, historians need to work in a more historical way. If gender identity is based only and universally on fear of castration, the point of historical inquiry is denied. Moreover, real men and women do not always or literally fulfill the terms of their society's prescriptions or of our analytic categories. Historians need instead to examine the ways in which gendered identities are substantively constructed and relate their findings to a range of activities, social organizations, and historically specific cultural representations. The best efforts in this area so far have been, not surprisingly, biographies: Biddy

[37] For this argument, see Rubin, "Traffic in Women," 199.
[38] Rubin, "Traffic in Women," 189.

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Martin's interpretation of Lou Andreas Salomé, Kathryn Sklar's depiction of Catharine Beecher, Jacqueline Hall's life of Jessie Daniel Ames, and Mary Hill's discussion of Charlotte Perkins Gilman.[39] But collective treatments are also possible, as Mrinalini Sinha and Lou Ratté have shown in their respective studies of the terms of construction of gender identity for British colonial administrators in India and British-educated Indians who emerged as anti-imperialist, nationalist leaders.[40]

The first part of my definition of gender consists, then, of all four of these elements, and no one of them operates without the others. Yet they do not operate simultaneously, with one simply reflecting the others. A question for historical research is, in fact, what the relationships among the four aspects are. The sketch I have offered of the process of constructing gender relationships could be used to discuss class, race, ethnicity, or, for that matter, any social process. My point was to clarify and specify how one needs to think about the effect of gender in social and institutional relationships, because this thinking is often not done precisely or systematically. The theorizing of gender, however, is developed in my second proposition: gender is a primary way of signifying relationships of power. It might be better to say, gender is a primary field within which or by means of which power is articulated. Gender is not the only field, but it seems to have been a persistent and recurrent way of enabling the signification of power in the West, in Judeo-Christian as well as Islamic traditions. As such, this part of the definition might seem to belong in the normative section of the argument, yet it does not, for concepts of power, though they may build on gender, are not always literally about gender itself. French sociologist Pierre Bourdieu has written about how the "di-vision du monde," based on references to "biological differences and notably those that refer to the division of the labor of procreation and reproduction," operates as "the best-founded of collective illusions." Established as an objective set of references, concepts of gender structure perception and the concrete and symbolic organization of all social life.[41] To the extent that these references establish distributions of power (differential control over or access to material and symbolic resources), gender becomes implicated in the conception and construction of power itself. The French anthropologist Maurice Godelier has put it this way: "It is not sexuality which haunts society, but society which haunts the body's sexuality. Sex-related differences between bodies are continually summoned as testimony to social relations and phenomena that have nothing to do with sexuality. Not only as testimony to, but also testimony for—in other words, as legitimation."[42]

[39] Biddy Martin, "Feminism, Criticism and Foucault," *New German Critique*, 27 (Fall 1982): 3–30; Kathryn Kish Sklar, *Catharine Beecher: A Study in American Domesticity* (New Haven, Conn., 1973); Mary A. Hill, *Charlotte Perkins Gilman: The Making of a Radical Feminist, 1860–1896* (Philadelphia, 1980).

[40] Lou Ratté, "Gender Ambivalence in the Indian Nationalist Movement," unpublished paper, Pembroke Center Seminar, Spring 1983; and Mrinalini Sinha, "Manliness: A Victorian Ideal and the British Imperial Elite in India," unpublished paper, Department of History, State University of New York, Stony Brook, 1984.

[41] Pierre Bourdieu, *Le Sens Pratique* (Paris, 1980), 246–47, 333–461, especially 366.

[42] Maurice Godelier, "The Origins of Male Domination," *New Left Review*, 127 (May–June 1981): 17.

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The legitimizing function of gender works in many ways. Bourdieu, for example, showed how, in certain cultures, agricultural exploitation was organized according to concepts of time and season that rested on specific definitions of the opposition between masculine and feminine. Gayatri Spivak has done a pointed analysis of the uses of gender in certain texts of British and American women writers.[43] Natalie Davis has shown how concepts of masculine and feminine related to understandings and criticisms of the rules of social order in early modern France.[44] Historian Caroline Bynum has thrown new light on medieval spirituality through her attention to the relationships between concepts of masculine and feminine and religious behavior. Her work gives us important insight into the ways in which these concepts informed the politics of monastic institutions as well as of individual believers.[45] Art historians have opened a new territory by reading social implications from literal depictions of women and men.[46] These interpretations are based on the idea that conceptual languages employ differentiation to establish meaning and that sexual difference is a primary way of signifying differentiation.[47] Gender, then, provides a way to decode meaning and to understand the complex connections among various forms of human interaction. When historians look for the ways in which the concept of gender legitimizes and constructs social relationships, they develop insight into the reciprocal nature of gender and society and into the particular and contextually specific ways in which politics constructs gender and gender constructs politics.

POLITICS IS ONLY ONE OF THE AREAS IN WHICH GENDER can be used for historical analysis. I have chosen the following examples relating to politics and power in their most traditionally construed sense, that is, as they pertain to government and the nation-state, for two reasons. First, the territory is virtually uncharted, since gender has been seen as antithetical to the real business of politics. Second, political history—still the dominant mode of historical inquiry—has been the stronghold of resistance to the inclusion of material or even questions about women and gender.

Gender has been employed literally or analogically in political theory to justify or criticize the reign of monarchs and to express the relationship between ruler and ruled. One might have expected that the debates of contemporaries over the

[43] Gayatri Chakravorty Spivak, "Three Women's Texts and a Critique of Imperialism," *Critical Inquiry*, 12 (Autumn 1985): 243–46. See also Kate Millett, *Sexual Politics* (New York, 1969). An examination of how feminine references work in major texts of Western philosophy is carried out by Luce Irigaray in *Speculum of the Other Woman* (Ithaca, N.Y., 1985).

[44] Natalie Zemon Davis, "Women on Top," in her *Society and Culture in Early Modern France* (Stanford, Calif., 1975), 124–51.

[45] Caroline Walker Bynum, *Jesus as Mother: Studies in the Spirituality of the High Middle Ages* (Berkeley, Calif., 1982); Caroline Walker Bynum, "Fast, Feast, and Flesh: The Religious Significance of Food to Medieval Women," *Representations*, 11 (Summer 1985): 1–25; Caroline Walker Bynum, "Introduction," *Religion and Gender: Essays on the Complexity of Symbols* (forthcoming, Beacon Press, 1987).

[46] See, for example, T. J. Clarke, *The Painting of Modern Life* (New York, 1985).

[47] The difference between structuralist and post-structuralist theorists on this question rests on how open or closed they view the categories of difference. To the extent that post-structuralists do not fix a universal meaning for the categories or the relationship between them, their approach seems conducive to the kind of historical analysis I am advocating.

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reigns of Elizabeth I in England and Catharine de Medici in France would dwell on the issue of women's suitability for political rule, but, in the period when kinship and kingship were integrally related, discussions about male kings were equally preoccupied with masculinity and femininity.[48] Analogies to the marital relationship provide structure for the arguments of Jean Bodin, Robert Filmer, and John Locke. Edmund Burke's attack on the French Revolution is built around a contrast between ugly, murderous *sans-culottes* hags ("the furies of hell, in the abused shape of the vilest of women") and the soft femininity of Marie-Antoinette, who escaped the crowd to "seek refuge at the feet of a king and husband" and whose beauty once inspired national pride. (It was in reference to the appropriate role for the feminine in the political order that Burke wrote, "To make us love our country, our country ought to be lovely.")[49] But the analogy is not always to marriage or even to heterosexuality. In medieval Islamic political theory, the symbols of political power alluded most often to sex between man and boy, suggesting not only forms of acceptable sexuality akin to those that Foucault's last work described in classical Greece but also the irrelevance of women to any notion of politics and public life.[50]

Lest this last comment suggest that political theory simply reflects social organization, it seems important to note that changes in gender relationships can be set off by views of the needs of state. A striking example is Louis de Bonald's argument in 1816 about why the divorce legislation of the French Revolution had to be repealed:

> Just as political democracy "allows the people, the weak part of political society, to rise against the established power," so divorce, "veritable domestic democracy," allows the wife, "the weak part, to rebel against marital authority" . . . "In order to keep the state out of the hands of the people, it is necessary to keep the family out of the hands of wives and children."[51]

Bonald begins with an analogy and then establishes a direct correspondence between divorce and democracy. Harking back to much earlier arguments about the well-ordered family as the foundation of the well-ordered state, the legislation

[48] Rachel Weil, "The Crown Has Fallen to the Distaff: Gender and Politics in the Age of Catharine de Medici," *Critical Matrix* (Princeton Working Papers in Women's Studies), 1 (1985). See also Louis Montrose, "Shaping Fantasies: Figurations of Gender and Power in Elizabethan Culture," *Representations*, 2 (Spring 1983): 61–94; and Lynn Hunt, "Hercules and the Radical Image in the French Revolution," *Representations*, 2 (Spring 1983): 95–117.

[49] Edmund Burke, *Reflections on the French Revolution* (1892; reprint edn., New York, 1909), 208–09, 214. See Jean Bodin, *Six Books of the Commonwealth* (1606; reprint edn., New York, 1967); Robert Filmer, *Patriarcha and Other Political Works*, ed. Peter Laslett (Oxford, 1949); and John Locke, *Two Treatises of Government* (1690; reprint edn., Cambridge, 1970). See also Elizabeth Fox-Genovese, "Property and Patriarchy in Classical Bourgeois Political Theory," *Radical History Review*, 4 (Spring–Summer 1977): 36–59; and Mary Lyndon Shanley, "Marriage Contract and Social Contract in Seventeenth Century English Political Thought," *Western Political Quarterly*, 32 (March 1979): 79–91.

[50] I am grateful to Bernard Lewis for the reference to Islam. Michel Foucault, *Histoire de la Sexualité*, vol. 2, *L'Usage des plaisirs* (Paris, 1984). One wonders in situations of this kind what the terms of the subject's gender identity are and whether Freudian theory is sufficient to describe the process of its construction. On women in classical Athens, see Marilyn Arthur, "Liberated Woman: The Classical Era," in Renate Bridenthal and Claudia Koonz, eds., *Becoming Visible* (Boston, 1976), 75–78.

[51] Cited in Roderick Phillips, "Women and Family Breakdown in Eighteenth Century France: Rouen 1780–1800," *Social History*, 2 (May 1976): 217.

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that implemented this view redefined the limits of the marital relationship. Similarly, in our own time, conservative political ideologues would like to pass a series of laws about the organization and behavior of the family that would alter current practices. The connection between authoritarian regimes and the control of women has been noted but not thoroughly studied. Whether at a crucial moment for Jacobin hegemony in the French Revolution, at the point of Stalin's bid for controlling authority, the implementation of Nazi policy in Germany, or the triumph in Iran of the Ayatollah Khomeni, emergent rulers have legitimized domination, strength, central authority, and ruling power as masculine (enemies, outsiders, subversives, weakness as feminine) and made that code literal in laws (forbidding women's political participation, outlawing abortion, prohibiting wage-earning by mothers, imposing female dress codes) that put women in their place.[52] These actions and their timing make little sense in themselves; in most instances, the state had nothing immediate or material to gain from the control of women. The actions can only be made sense of as part of an analysis of the construction and consolidation of power. An assertion of control or strength was given form as a policy about women. In these examples, sexual difference was conceived in terms of the domination or control of women. These examples provide some insight into the kinds of power relationships being constructed in modern history, but this particular type of relationship is not a universal political theme. In different ways, for example, the democratic regimes of the twentieth century have also constructed their political ideologies with gendered concepts and translated them into policy; the welfare state, for example, demonstrated its protective paternalism in laws directed at women and children.[53] Historically, some socialist and anarchist movements have refused metaphors of domination entirely, imaginatively presenting their critiques of particular regimes or social organizations in terms of transformations of gender identities. Utopian socialists in France and England in the 1830s and 1840s conceived their dreams for a harmonious future in terms of the complementary natures of individuals as exemplified in the union of man and woman, "the social individual."[54] European anarchists were long known not only for refusing the conventions of bourgeois marriage but also for their visions of a world in which sexual difference did not imply hierarchy.

These examples are of explicit connections between gender and power, but they are only a part of my definition of gender as a primary way of signifying

[52] On the French Revolution, see Darlene Gay Levy, Harriet Applewhite, and Mary Johnson, eds., *Women in Revolutionary Paris, 1789–1795* (Urbana, Ill., 1979), 209–20; on Soviet legislation, see the documents in Rudolph Schlesinger, *The Family in the USSR: Documents and Readings* (London, 1949), 62–71, 251–54; on Nazi policy, see Tim Mason, "Women in Nazi Germany," *History Workshop*, 1 (Spring 1976): 74–113, and Tim Mason, "Women in Germany, 1925–40: Family, Welfare and Work," *History Workshop*, 2 (Autumn 1976): 5–32.

[53] Elizabeth Wilson, *Women and the Welfare State* (London, 1977); Jane Jenson, "Gender and Reproduction"; Jane Lewis, *The Politics of Motherhood: Child and Maternal Welfare in England 1900–1939* (Montreal, 1980); Mary Lynn McDougall, "Protecting Infants: The French Campaign for Maternity Leaves, 1890s–1913," *French Historical Studies*, 13 (1983): 79–105.

[54] On English utopians, see Barbara Taylor, *Eve and the New Jerusalem* (New York, 1983); on France, Joan W. Scott, "Men and Women in the Parisian Garment Trades: Discussions of Family and Work in the 1830s and 40s," in Pat Thane, *et al.*, eds., *The Power of the Past: Essays for Eric Hobsbawm* (Cambridge, 1984), 67–94.

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relationships of power. Attention to gender is often not explicit, but it is nonetheless a crucial part of the organization of equality or inequality. Hierarchical structures rely on generalized understandings of the so-called natural relationship between male and female. The concept of class in the nineteenth century relied on gender for its articulation. When middle-class reformers in France, for example, depicted workers in terms coded as feminine (subordinated, weak, sexually exploited like prostitutes), labor and socialist leaders replied by insisting on the masculine position of the working class (producers, strong, protectors of their women and children). The terms of this discourse were not explicitly about gender, but they relied on references to it, the gendered "coding" of certain terms, to establish their meanings. In the process, historically specific, normative definitions of gender (which were taken as givens) were reproduced and embedded in the culture of the French working class.[55]

The subject of war, diplomacy, and high politics frequently comes up when traditional political historians question the utility of gender in their work. But here, too, we need to look beyond the actors and the literal import of their words. Power relations among nations and the status of colonial subjects have been made comprehensible (and thus legitimate) in terms of relations between male and female. The legitimizing of war—of expending young lives to protect the state—has variously taken the forms of explicit appeals to manhood (to the need to defend otherwise vulnerable women and children), of implicit reliance on belief in the duty of sons to serve their leaders or their (father the) king, and of associations between masculinity and national strength.[56] High politics itself is a gendered concept, for it establishes its crucial importance and public power, the reasons for and the fact of its highest authority, precisely in its exclusion of women from its work. Gender is one of the recurrent references by which political power has been conceived, legitimated, and criticized. It refers to but also establishes the meaning of the male/female opposition. To vindicate political power, the reference must seem sure and fixed, outside human construction, part of the natural or divine order. In that way, the binary opposition and the social process of gender relationships both become part of the meaning of power itself; to question or alter any aspect threatens the entire system.

If significations of gender and power construct one another, how do things change? The answer in a general sense is that change may be initiated in many places. Massive political upheavals that throw old orders into chaos and bring new ones into being may revise the terms (and so the organization) of gender in the search for new forms of legitimation. But they may not; old notions of gender have

[55] Louis Devance, "Femme, famille, travail et morale sexuelle dans l'idéologie de 1848," in *Mythes et représentations de la femme au XIX^e siècle* (Paris, 1976); Jacques Rancière and Pierre Vauday, "En allant à l'expo: l'ouvrier, sa femme et les machines," *Les Révoltes Logiques*, 1 (Winter 1975): 5–22.

[56] Gayatri Chakravorty Spivak, " 'Draupadi' by Mahasveta Devi," *Critical Inquiry*, 8 (Winter 1981): 381–402; Homi Bhabha, "Of Mimicry and Man: The Ambivalence of Colonial Discourse," *October*, 28 (Spring 1984): 125–33; Karin Hausen, "The Nation's Obligations to the Heroes' Widows of World War I," in Margaret R. Higonnet, *et al.*, eds., *Women, War and History* (New Haven, Conn., 1986). See also Ken Inglis, "The Representation of Gender on Australian War Memorials," unpublished paper presented at the Bellagio Conference on Gender, Technology and Education, October 1985.

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also served to validate new regimes.[57] Demographic crises, occasioned by food shortages, plagues, or wars, may have called into question normative visions of heterosexual marriage (as happened in some circles, in some countries in the 1920s), but they have also spawned pro-natalist policies that insist on the exclusive importance of women's maternal and reproductive functions.[58] Shifting patterns of employment may lead to altered marital strategies and to different possibilities for the construction of subjectivity, but they can also be experienced as new arenas of activity for dutiful daughters and wives.[59] The emergence of new kinds of cultural symbols may make possible the reinterpreting or, indeed, rewriting of the oedipal story, but it can also serve to reinscribe that terrible drama in even more telling terms. Political processes will determine which outcome prevails—political in the sense that different actors and different meanings are contending with one another for control. The nature of that process, of the actors and their actions, can only be determined specifically, in the context of time and place. We can write the history of that process only if we recognize that "man" and "woman" are at once empty and overflowing categories. Empty because they have no ultimate, transcendent meaning. Overflowing because even when they appear to be fixed, they still contain within them alternative, denied, or suppressed definitions.

Political history has, in a sense, been enacted on the field of gender. It is a field that seems fixed yet whose meaning is contested and in flux. If we treat the opposition between male and female as problematic rather than known, as something contextually defined, repeatedly constructed, then we must constantly ask not only what is at stake in proclamations or debates that invoke gender to explain or justify their positions but also how implicit understandings of gender are being invoked and reinscribed. What is the relationship between laws about women and the power of the state? Why (and since when) have women been invisible as historical subjects, when we know they participated in the great and small events of human history? Has gender legitimated the emergence of professional careers?[60] Is (to quote the title of a recent article by French feminist Luce Irigaray) the subject of science sexed?[61] What is the relationship between state

[57] On the French Revolution, see Levy, *Women in Revolutionary Paris*; on the American Revolution, see Mary Beth Norton, *Liberty's Daughters: The Revolutionary Experience of American Women* (Boston, 1980); Linda Kerber, *Women of the Republic* (Chapel Hill, N.C., 1980); Joan Hoff-Wilson, "The Illusion of Change: Women and the American Revolution," in Alfred Young, ed., *The American Revolution: Explorations in the History of American Radicalism* (DeKalb, Ill., 1976), 383–446. On the French Third Republic, see Steven Hause, *Women's Suffrage and Social Politics in the French Third Republic* (Princeton, N.J., 1984). An extremely interesting treatment of a recent case is Maxine Molyneux, "Mobilization without Emancipation? Women's Interests, the State and Revolution in Nicaragua," *Feminist Studies*, 11 (Summer 1985): 227–54.

[58] On pro-natalism, see Riley, *War in the Nursery*; and Jenson, "Gender and Reproduction." On the 1920s, see the essays in *Strategies des Femmes* (Paris, 1984).

[59] For various interpretations of the impact of new work on women, see Louise A. Tilly and Joan W. Scott, *Women, Work and Family* (New York, 1978); Thomas Dublin, *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826–1860* (New York, 1979); and Edward Shorter, *The Making of the Modern Family* (New York, 1975).

[60] See, for example, Margaret Rossiter, *Women Scientists in America: Struggles and Strategies to 1914* (Baltimore, Md., 1982).

[61] Luce Irigaray, "Is the Subject of Science Sexed?" *Cultural Critique*, 1 (Fall 1985): 73–88.

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politics and the discovery of the crime of homosexuality?[62] How have social institutions incorporated gender into their assumptions and organizations? Have there ever been genuinely egalitarian concepts of gender in terms of which political systems were projected, if not built?

Investigation of these issues will yield a history that will provide new perspectives on old questions (about how, for example, political rule is imposed, or what the impact of war on society is), redefine the old questions in new terms (introducing considerations of family and sexuality, for example, in the study of economics or war), make women visible as active participants, and create analytic distance between the seemingly fixed language of the past and our own terminology. In addition, this new history will leave open possibilities for thinking about current feminist political strategies and the (utopian) future, for it suggests that gender must be redefined and restructured in conjunction with a vision of political and social equality that includes not only sex, but class and race.

[62] Louis Crompton, *Byron and Greek Love: Homophobia in Nineteenth-Century England* (Berkeley, Calif., 1985). This question is touched on in Jeffrey Weeks, *Sex, Politics and Society* (New York, 1983).

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Japanese *Gimin*: Peasant Martyrs in Popular Memory

ANNE WALTHALL

SINCE 1965, JAPANESE FARMERS HAVE PROTESTED the plans of the government to build the New Tokyo (Narita) International Airport. Joined by students, workers, and housewives, the farmers have chained themselves to trees, doused riot police with urine and feces, built watchtowers, fortresses, bunkers, and foxholes, thrown Molotov cocktails, and blown up the airport's control tower to protest the rape of their soil. Three centuries before, a peasant named Sakura Sōgorō died on a cross for having petitioned the authorities for famine relief. Sōgorō humbly begged his liege lord to lower taxes. Pilgrims who throng Sōgorō's shrine worship him as a beneficient deity, the spiritual guardian of peace and harmony for the household and the state. Yet the activist farmers who oppose the arbitrary decision to seize their land and destroy their environment for the sake of progress claim, "We are the descendants of Sōgorō."[1] For many Japanese, the story of Sōgorō is one means by which a sense of the past is revived, codified, and used. Although they differ over how to interpret this past, it still remains in the collective memory of ordinary citizens who have recounted his story, worshiped at his grave, and, in modern times, lauded him as the greatest of Japan's *gimin*, the self-sacrificing peasant heroes.

Sōgorō is but one of Japan's many peasant martyrs who have been consciously and collectively remembered in one form or another. With the exception of the island of Hokkaido, uninhabited by the Japanese until modern times, each of the sixty-six provinces of pre-modern Japan, from Mutsu to Kyushu, contains traces of efforts to memorialize peasant heroes. One leading scholar of *gimin*, Hosaka Satoru, has discovered almost three hundred instances in which the leaders of the approximately three thousand protests that occurred between 1600 and 1868 were

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[1] In their struggle over the airport, activists retrieved layers of legends from recent times to the early seventeenth century, including that of Sōgorō. See David E. Apter and Nagayo Sawa, *Against the State* (Cambridge, Mass., 1984), 37, 62–64. This is the only book in English to deal seriously with the modern protester's sense of the past. The shrine to Sakura Sōgorō is in Chiba-ken, Narita-shi, two stops short of Narita International Airport on the Keisei line.

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later glorified in monuments, memorial services, and stories.[2] Many did nothing more than present a petition for tax relief on behalf of their villages; a few led uprisings that engulfed a region. The ruling authorities executed these men for crimes against the state and did everything in their power to ensure that the criminals were forgotten. Peasants usually preferred to forget the people castigated for creating disorder, but, occasionally, they felt obliged to a man who had made their lives easier to bear or they feared that his unpropitiated spirit would wreak vengeance on their villages, and so they offered prayers in his memory. The age-old belief that the spirits of the dead must be consoled through prayer and memorial services is central to the *gimin* tradition. Without this religious dimension to the social production of memory, the leaders of peasant uprisings might well have been forgotten long before the modern period. But some were not, and people have continued to erect monuments to them and worship at their shrines.

Gimin represent specific conceptions of the past in contemporary Japan. Today, groups of citizens set up historical markers, refurbish old monuments, and dedicate elaborate new ones to the memories of *gimin*. They perform memorial services and mark important anniversaries. Stories about martyrs are the subject of school plays and children's stories. Histories of peasant uprisings have been retold in the form of modern novels. For the most part, people involved in efforts to recall past heroes live in towns and villages away from the major urban centers. To examine what they have done and to explore its significance, a scholar must travel to tiny shrines and roadside steles and consult pamphlets published by local memorial societies—the byways of Japanese culture and history.

A focus on *gimin* provides a historical perspective on the ambiguities inherent in those symbols that invoke the past to supplement and subvert the emphases of the national heritage in fulfilling the needs of ordinary citizens. Just as not all the tens of thousands of villages in Japan were scenes of disturbances, so not all Japanese construct their personal heritage around dead heroes. Some satisfy their nostalgia by patronizing folk crafts, others by commemorating poets or local intellectuals.[3] In the Tokugawa period (1600–1868), peasants usually had to defy or subvert the intent of governmental regulations if they were to remember their fallen leaders. For American historians of this period, such as W. Donald Burton and Irwin Scheiner, these forms of remembrance have suggested either a spirit of

[2] Hosaka Satoru, "Gimin nendai hyō," *Tōei: Hongō Kotogakkō kiyō*, 18 (1985): 41. Hosaka has done more than any other scholar to collect references to and narratives of *gimin*. Thanks to his guidance, I was able to see many of the pamphlets, steles, and shrines referred to in this article, sometimes with results surprising to both of us. For example, we learned of the wrangling over the shrine to Kasuke, when I visited the site in 1983 and talked to the keeper of the meeting hall.

[3] "Just as Onta potters make use of the ideas of tradition to protect their community from many of the outside influences upsetting their social organization, so the Japanese as a whole have gone back to their traditional arts in order to preserve a national identity in the face of cultural innovations from the west." Brian Moeran, *Lost Innocence: Folk Craft Potters of Onta, Japan* (Berkeley, Calif., 1984), 224. A monument was erected in 1941 to commemorate Suzuki Bokushi (1770–1842), a poet and local intellectual from Niigata prefecture. In 1960, his hometown of Shiozawa established a Bokushi memorial society, which issued a collection of his works and his biography the following year. Takahashi Minoru, *Hokuetsu seppu no shisō* (Niigata-shi, 1981), 349.

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opposition or an unwritten covenant between peasants and rulers.[4] But these historians have not examined the modern period, when, during the struggle for popular rights (*jiyū minken undō*), peasant martyrs became political symbols, symbols later put to a variety of uses by both liberals and the extreme right. This exercise in the study of popular memory exemplifies one way in which subordinated social groups and communities develop and maintain a sense of their own history, as well as how this consciousness changes, the extent and form of its autonomy, and the political purposes for which it can be mobilized. This study has an explicitly cultural focus in that it deals with a specifically Japanese image, the *gimin*, but one whose appeal could be compared to that of Marianne in France or the *soldaderas* in Mexico and the American Southwest, which have also undergone changes as images of political and social identification.[5]

FOR MUCH OF THE TOKUGAWA PERIOD, the shogun and the some 265 lords (*daimyō*) who ruled Japan tried to erase all signs of uprisings and disorder. The leaders of protest were treated as common criminals; they died in prison or on the execution ground, their corpses cast in burial pits for horses. After an uprising in 1704 in the Mito domain, officials destroyed the memoranda concerning the riot and encouraged village officials to do the same.[6] Those who gossiped about past events were reported by government spies and faced severe penalties. The degree to which these rules were enforced varied from domain to domain and from one instance to another, and the degree to which peasants obeyed them varied even more. Nonetheless, rulers intended to obliterate the means that the common people used to recall deeds of protest. Deeply embarrassed by disorder in their domains, local rulers did not want news of their difficulties to spread. They may have wanted to prevent the rise of practices that would sustain political criticism and reinforce modes of political action for people whom they considered to be outside politics altogether. During uprisings, peasants discussed with each other the domanial policy and qualifications for office that warriors jealously held to be their own prerogative. Perhaps the rulers feared that if peasants were allowed to reflect on their experience, they might start to demand some degree of participation in decision-making councils or assert, as they in fact did, that they were the

[4] W. Donald Burton, "Peasant Struggle in Japan, 1590–1760," *Journal of Peasant Studies*, 5 (1978): 151–52; Irwin Scheiner, "Benevolent Lords and Honorable Peasants: Rebellion and Peasant Consciousness in Tokugawa Japan," in Tetsuo Najita and Irwin Scheiner, eds., *Japanese Thought in the Tokugawa Period* (Chicago, 1978), 50–52.

[5] This approach is based on my reading of Maurice Agulhon, *Marianne into Battle: Republican Imagery and Symbolism in France, 1789–1880* (Cambridge, 1981) and Richard Johnson, *et al.*, eds., *Making Histories: Studies in History Writing and Politics* (Minneapolis, Minn., 1982). A comprehensive study of the *soldaderas* (female soldiers and campfollowers) is in Elizabeth Salas, "Las Adelitas, from Soldaderas to Chicano Feminists: Changing Patterns of an Identity Symbol" (Ph.D. dissertation, University of California, Los Angeles, 1986).

[6] Egawa Fumihiro, *Hoei ikki: Mito han o yurugaseta hyakushōtachi* (Ibaraki-ken, Tsuchiura-shi, 1981), 125–34.

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equals of the warriors.[7] Free and open discussion could also give peasants a model to follow in the future. For these reasons, administrators threatened severe reprisals against anyone caught openly honoring the deeds of peasant martyrs.

Rulers did not suppress every mention of leaders of uprisings, however. Early on, peasant martyrs became symbols of righteousness, symbols so fluid that the authorities could appropriate them for their own purposes. In 1746, almost one hundred years after Sōgorō's execution, the lord of the Sakura domain, troubled, so he said, by Sōgorō's unquiet ghost, gave Sōgorō's descendants a stipend. Having fallen into poverty in 1806, the descendants received a grant of over an acre and a half of low-grade paddy. On the 150th anniversary of Sōgorō's death, the ruling family erected a tombstone in his honor and glorified him with a high-ranking name.[8] A prominent scholar of Japanese peasant uprisings, Fukaya Katsumi, has argued that ruling authorities worshiped peasant martyrs as gods (*kami*) to render them harmless and transform them into instruments of control.[9] No doubt the rulers shared the peasants' belief in the power of angry spirits (*onryō*), but the authorities may also have wanted to laud the respectful and self-sacrificial actions of leaders like Sōgorō at a time when peasant protest was becoming increasingly aggressive and violent. Officials praised only a few solitary petitioners whom they themselves selected. They permitted their vision of righteous heroes to gain publicity, but the peasants' vision had to remain clandestine.

Building simple grave markers for the leaders of uprisings had to be done in secret. One hundred years after the execution of six men in 1635 for opposing a land survey near Gotenba, a stone carved with honorific names given to them posthumously was hidden in the forest on a hill above and behind a small shrine.[10] The graves of men who led a series of protests in the 1770s against the resident intendant (*daikan*), Ōhara Hikokurō, remain scattered throughout the area around Takayama in Gifu. Some markers, like that of Heibei, whose gravestone was erected in 1789, and that of Kaneoka Jinzō, have recently been moved from obscure to prominent positions.[11] Throughout the intervening years, worshipers kept alive the memory of what these men had done and offered prayers to ease their spirits.

Monuments to fallen leaders blur the distinction historians of Japanese religion usually make between gravestones and memorial tablets (*kuyō-hi*). Family members erected the gravestones, and worship of the dead man's spirit was confined to his descendants. Memorial tablets required a special effort and were often purchased by nonrelatives. It might be argued that men for whom simple grave markers were erected ought not to be considered martyrs because no more was done to exalt their memory. But peasants built these graves in defiance of the authorities, and, in most

[7] The leader of an uprising in the Amakusa islands, Nagata Ryūsaburō, built two monuments in 1828 that proclaimed, "All praise to lord Amida, all are equal without distinction in the world of his law" ("namu Amida butsu, muen hōkai byōdō"). Kumamoto-ken, Amakusa-gun.

[8] Kodama Kōta, *Sakura Sōgorō* (Tokyo, 1958), 157–74; see also Scheiner, "Benevolent Lords and Honorable Peasants," 48.

[9] Fukaya Katsumi, *Hyakushō ikki no rekishiteki kōzō* (Tokyo, 1979), 280.

[10] Nogi Tatsuo, "Mikuriya ikki no ato o tazunete," *Shishi no sanpo michi*, 4 (March 1974): 5, 7.

[11] Gifu-ken, Takayama-shi, Gansho-ji; Gifu-ken, Kofuku-chō, Kaneoka.

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cases, the graves provided a place for worship by people not of the martyr's family. What had begun as no more than a diffuse tendency in popular thought had, by the mid-eighteenth century, become crystallized in commemorative steles.

Peasants often camouflaged monuments to the leaders of uprisings as something else. When the descendants of Shōzaemon (executed in Obama in 1652) erected a stele to his memory in 1774, they falsified information. The history engraved on the stone indeed told how Shōzaemon had petitioned nine times to get the measure used for the bean tax reduced to its former and smaller size, but in this account the event was placed in the sixteenth century instead of the Tokugawa period because, otherwise, the monument could have been taken as a criticism of the current ruling family.[12] Displacing events was a common ploy to avoid censure: the play *The Treasury of Loyal Retainers* (*Chūshingura*) situated an early eighteenth-century vendetta in the fourteenth century to deflect the wrath of the authorities. Other memorials received other disguises. A road sign erected in 1823 was, in reality, a monument to leaders executed in 1804 in an uprising in Ibaraki; their names were carved at the bottom.[13] A number of statues in Shikoku, particularly in Ehime prefecture, were obstensibly done as religious figures, such as *Jizō*, the guardian deity for children, but the deities associated with them were the spirits of peasant martyrs.[14] Matsui Ichibei, Saburōemon, and Haritsuke Mozaemon, crucified in 1681 following a petition for a tax reduction to the Numata domain lord, were worshiped in the form of *Jizō*. Small stone *Jizō* dot the Japanese countryside, but, as these examples indicate, some of them had a private significance known only to local peasants.

The contest between ruling authorities and peasants over whether or not martyrs would be commemorated could itself become the stuff of legends. Peasants on Sado erected a memorial to Tarōemon after he was executed for leading an uprising in 1749, but shogunal officials regarded it as "disrespectful" and destroyed it. In 1840, Shinbei of Takeda village, also on Sado, erected a five-story stone pagoda as a memorial to Zenbei and others who died in an Edo prison after a riot in 1838. Shogunal officials sent men to demolish the pagoda, but, forewarned, Shinbei and his confederates secreted it at the base of a pine

[12] Kawamura Jinuemon, *Wakasa no gimin: Gimin Shōzaemon to toji no Wakasa nōsei* (1932–33; reprint edn., Fukui-ken, Kaminaka-chō, 1970), 123. Yokoyama Toshio discussed this publication as an example of how local historians have uncovered histories of the past missing in official records. Yokoyama Toshio, *Gimin: Hyakushō ikki no shidōtachi* (Tokyo, 1973), 36–37. Yokoyama has gained national recognition through his writings on *gimin*. He has traced the history of *gimin* from the protests they led to their modern uses, unearthing tremendous amounts of information in his efforts to sift the truth of what happened from later legendary accretions. I argue that it is precisely in these accretions that their significance as symbol is to be found. Furthermore, *gimin*, especially in the modern period, must be viewed in broader terms than simply their representation of the indigenous roots for a democratic tradition in Japan. See Yokoyama Toshio, *Hyakushō ikki to gimin denshō* (Tokyo, 1977); Yokoyama Toshio, *Gimin denshō no kenkyū* (Tokyo, 1985); and Yokoyama Toshio, *Shinano no hyakushō ikki to gimin denshō* (Matsumoto-shi, 1986).

[13] Ibaraki-ken, Ami-chō, Suzuki Hisashi, *Koyaku: Joku kantan yume monogatari* (Ibaraki-ken, Tsuchiura-shi, 1979), 6.

[14] Hosaka Satoru, "Gimin denshō no keisei to tenkai," *Rekishi e no shōtai*, 19 (1981): 51. A martyr in the 1681 Shōnai protest has been worshiped as Amida Buddha. William W. Kelly, *Deference and Defiance in Nineteenth-Century Japan* (Princeton, N.J., 1985), 57. Although Kelly noted several instances where peasants commemorated the deeds of leaders of uprisings, he did not discuss their significance.

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surrounded by a bamboo grove. Arrested and tortured to reveal the whereabouts of the monument and the names of his helpers, Shinbei was alleged to have protected it and them to the death. Another peasant erected a grave marker to Shinbei beside the original pagoda.[15]

The extreme steps sometimes taken by the governmental authorities to discover and destroy monuments erected in memory of peasant martyrs indicate the seriousness with which they viewed infractions of their regulations and attempts to remember acts of protest. In their efforts to retain hegemony over the representation of history, they disqualified—except for fragments found in official versions—the experience of protest from the history they had constructed to be publicly relevant. This disallowed but unerasable past did not lose its value as social experience for the peasants, but, for those in Shikoku, it was excluded from the dominant system of communication. For peasants near Gotenba and Takayama, it became private, and for peasants in Sado, it was distorted in myth. Fluid and elusive, the symbolic use of peasant martyrs catches the historian's eye only when its traces remain concrete—as steles or, perhaps, periodic memorial services and compelling legends.

Stone monuments were often the focal point of yearly or regularly timed religious ceremonies during which peasants prayed to the spirits of their departed leaders, exalting them as powerful Shintō gods. Records from Aoki village in Nagano prefecture show that a festival for worshiping Hirabayashi Shinshichi (executed in 1721) as a goddess of rice (Inari Daimyōjin) began in 1767.[16] Kawaguchi Ichirōbei (crucified for resisting a land survey near Odawara in 1681) was worshiped as a crucified god of war (Haritsuke Hachiman).[17] In 1775, peasants near Gotenba built a small shrine to six men executed in 1635 and worshiped them as six gods (rokusō Daigongen).[18] Hachiman was the god of war and the tutelary deity of the first shogun family, the Minamoto. The Daimyōjin (or "divine rectifier") stood at the apex of Shintō deities. Daigongen (or "the ultimate made manifest") was the title by which Tokugawa Ieyasu (1542–1616) was commonly called by lords and commoners alike.[19] Early in the 1600s, these terms were ranked to accord with the political order. Powerful rulers became high-ranking gods (*kami*) after they died, but ordinary peasants barely qualified as dead spirits (*tama*). This

[15] Itō Harukazu, *Sado gimin den* (Niigata-ken, Sado-gun Shinhō-mura, 1938), 6, 155. A slightly different version of this incident is given by Hayashi Motoi, *Hyakushō ikki no dentō* (Tokyo, 1955), 279–80. Near Utsunomiya lies the grave of Gennojō, executed in 1753 for having protested a departure from customary tax collection policies. Someone erected a gravestone for him in the late eighteenth century, but the donor's name was chiseled off the stone. After further trouble with the officials, the stone was split in two. Tochigi-ken, Utsunomiya-shi, Obachōjima, "Utsunomiya girōden," manuscript in the Suzuki family collection.

[16] Nagano-ken, Aoki mura. The shrine I saw is tiny, no more than three feet high, perched on a rock in the woods behind the village. Inside is an empty wooden box, the receptacle for the body of the god labeled "Inari Daimyōjin," and a wooden plaque listing the people who have taken charge of the yearly ceremonies. For the story of Shinshichi, see Scheiner, "Benevolent Lords and Honorable Peasants," 49.

[17] Kohase Kotarō, "Haritsuke Hachimon no ichikōsatsu," *Yoshiwara chihōshi kenkyūkai kyōdo kenkyūshi*, 5 (1966): 95–99.

[18] Shizuoka-ken, Gotenba-shi, Furusawa.

[19] Conrad Totman, *Tokugawa Ieyasu: Shogun* (San Francisco, 1983), 189.

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Memorial monument (*kuyō-hi*) erected in the early nineteenth century, in memory of Zenkurō and Kijūrō, leaders of the Ohara dispute, a movement in the 1770s and 1790s to rid the region around Takayama of an evil intendant. Author's photograph.

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pecking order of the gods was created by the ruling authorities to maintain, support, and reflect status distinctions in the real world. In the case of peasant martyrs at least, peasants accepted the notion of an order of ranks but put their leaders at the top. In spirit, if not in flesh, the six peasants executed in 1635 were the equals of the great Ieyasu.

What did the peasants think when they worshiped a peasant martyr as a Shintō god? Yasumaru Yoshio, a historian who studies popular consciousness, has argued that because the peasants abhorred social disorder, they tried to forget about events of protest. Whether out of fear of angry spirits or gratitude for having been rescued from an impossible situation, however, they continued to be troubled by memories of their executed leaders, memories exorcized in seasonal ceremonies. By worshiping these men, the peasants transformed them into awesome, beneficient deities to whom they prayed for good harvests. Given the longstanding practice in Japan of making local heroes into local gods, the martyrs' heroic self-sacrifice on behalf of the village made it logical for villagers to assume that they continued to protect their people after death. Gradually, the details of what the martyrs had done became blurred or forgotten; only their role as guardians remained.[20] Yet peasants also constructed another sense of the past around martyrs by telling stories about their opposition to ruling officials. After the bean harvest in Obama, for example, the first fruits would be presented to the spirit of Matsuki Shōzaemon, enshrined on the god shelf (*kamidana*) of every household, and legends of his bravery would be told to children.[21] Tarōemon of Sado, Haritsuke Mozaemon, and the leaders of an uprising in Awaji became the subjects of legends repeated to appease their spirits.[22]

The legend of Sakura Sōgorō is the most famous example of the tales of peasant leaders that were retold in worship.[23] The legend began by announcing the reason for telling the tale: to explain the origin of a shrine. This explanation required recounting the evil deeds of Hotta Masanobu, lord of Sakura, who assessed heavy taxes, tyrannized the people, and drove them into destitution. Conscious of his duty as village headman to succor the poor and preserve the peace, Sōgorō rallied other headmen to appeal for tax reform and famine relief. Together, they entreated the chief magistrate and high officials in the Sakura domain to reduce taxes, but they were refused. They went to Edo and begged for help from the lord. They even petitioned members of the shogun's highest councils. Finally, Sōgorō decided to appeal alone directly to the shogun. By this audacious act, Sōgorō forced Masanobu to make concessions to the peasants, but Sōgorō bore the blame for having broken the law in going over local officials to higher authority. Angry

[20] Yasumaru Yoshio, *Nihon no kindaika to minshū shisō* (Tokyo, 1974), 149–50.

[21] Kawamura, *Wakasa no gimin*, 122; for legends about Shōzaemon, see Burton, "Peasant Struggle in Japan," 142, 151–52.

[22] For the legends about Tarōemon of Sado, see Itō, *Sado gimin den*, 55; for the leaders in Awaji, see Mori Shigeo, "Awaji gimin Miyamura Saizō," *Kamigata*, 97 (1939): 22–25; for Haritsuke Mōzaemon, see Yokoyama, *Gimin*, 20–29.

[23] In this way, the legend can be said to be part of a tradition of oral literature that includes such masterpieces as *Heike monogatari*. Herbert Plutschow, "The Fear of Evil Spirits in Japanese Culture," *Transactions of the Asiatic Society of Japan*, 3d ser., 18 (1983): 144–47.

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and embarrassed, Masanobu had Sōgorō and his wife crucified and his four children beheaded. Although Sōgorō accepted death for himself, he resented the punishment of his children, and he proclaimed his resolve to take vengeance on Masanobu. Masanobu finally went mad, and the Hotta family was forced to leave Sakura. To molify Sōgorō's angry spirit, people began to worship at his grave and recount tales of his deeds.[24]

In the eighteenth and nineteenth centuries, legends about Sōgorō and other leaders of uprisings became more elaborate and incorporated a variety of archetypal incidents. Matsuki Shōzaemon of Wakasa, Yōhei of Ueda, and Haritsuke Mozaemon of Numata were all said to have been executed just before pardons arrived from their lords.[25] In overall structure, these tales emphasized the rapacity of evil local officials, the heroic sacrifice of either ordinary peasants or, most often, village headmen, and the saving grace of rulers, presenting a construction of the past that had an enormously emotive, if ambiguous, appeal. According to Irwin Scheiner, these "morality plays" demonstrate that peasants believed and acted as if they lived in a world of justice ruled by a benevolent lord.[26] In written form, these stories were often transmitted by paternalistic village officials whose Confucian conception of their role led them to praise exemplars of their class. After large-scale uprisings in the eighteenth century, in which officials themselves were often attacked, they may also have used legends of self-sacrificing headmen to enhance their authority and promote their vision of the status quo. But it can also be argued that, depending on who did the telling, these legends went beyond the limits of morality plays. Insofar as legends depicted people who acted on behalf of the collectivity, they could also be told and used by illiterate or semi-literate peasants to force increasingly reluctant officials to act for them and to criticize by implication those who refused.

BY THE END OF THE TOKUGAWA PERIOD (1868), the image of peasant martyrs had divided further. The tradition of worshiping fallen leaders as the guardians of a region or as gods of good harvests continued but changed from within. Martyrs appeared in different guises. In a neverending search for novelty within stereotypic scenes of vengeance, Edo playwrights began to present peasants on urban stages. And, in a foreshadowing of modern political movements, the leaders

[24] Thirty manuscript versions of the Sōgorō story dating from the Tokugawa period have been found in peasant storehouses from central to northern Japan. Ōno Masaji, *Jizō-dō tsuya monogatari* (Chiba-ken, Ryūsan-shi, 1978). As befits the greatest of Japanese peasant heroes, Sōgorō has often been cited by historians. See Burton, "Peasant Struggles in Japan," 151; Scheiner, "Benevolent Lords and Honorable Peasants," 47–48. I mentioned in a previous article that angry ghosts find no place in post-Restoration accounts of peasant martyrs. Instead, political activists praised the fighting spirit of their ancestors, a spirit to be emulated. Anne Walthall, "Narratives of Peasant Uprisings in Japan," *Journal of Asian Studies*, 42 (1983): 576–79, 585. A modern version of Sogorō's story, one that omits the vengeful ghost, is in Michiko Y. Aoki and Margaret B. Dardess, eds., *As the Japanese See It: Past and Present* (Honolulu, 1981), 275–82.

[25] Scheiner, "Benevolent Lords and Honorable Peasants," 49; Burton, "Peasant Struggles in Japan," 151; and Yokoyama, *Gimin*, 24, 37, 168. According to Afrikaner political mythology, just after the British had executed some frontiersmen for the 1815 uprising, a reprieve for the condemned arrived. Desmond Tutu, "Mythology," *New York Review of Books* (September 26, 1985): 4.

[26] Scheiner, "Benevolent Lords and Honorable Peasants," 50.

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of protest called on local heroes not as spiritual protectors but as exemplars of "righteous men."

Changes in the worship of peasant martyrs suggest how village headmen, usually responsible for these ceremonies, had come to see themselves and their position in society. Scheiner has argued that, frustrated at their rulers' abrogation of benevolent responsibility for their subjects and their own lack of political power and status, peasant officials turned to the promise of nativism (*kokugaku*) for a mystical union with a transcendental emperor or, in some cases, for an imperial restoration in which their own importance would be recognized.[27] In 1863, Nakazawa Kidayū, headman of Seki village near Takasaki, composed a song to be accompanied by dancing at a special memorial service for Hyōnai, executed in 1766 for having protested a new requisition for post horses. "Well now, herein lies the tale of how the spirits of dead men and all things born between heaven and earth follow the inclination of their hearts to become one with the gods and buddhas," the song began. It cast Hyōnai in the role of a seventeenth-century hero who acted alone rather than recounting the riot that actually occurred, and it lyrically depicted his trip to Edo in the language of the travel passages (*michiyuki*) found in Japanese classics. Finally, the song offered thanks to the Imperial Court for having graciously raised Hyōnai's posthumous honorific rank.[28] In this instance, we can infer that the tradition of commemorating peasant martyrs enabled Kidayū to distort the past and assert the bond between his village and the emperor.

Amid increasing disorders during the 1850s and 1860s, some protest leaders called on the spirits of their predecessors to aid them in their endeavors. In 1859, peasants in the Sakura domain prayed to Sōgorō's spirit to kill five retainers who had forced them to pay bribes.[29] One leader of the Minamiyama uprising of 1853, Ogiso Ihei, built a shrine to Sōgorō and passed out talismans to his followers. These read: "Protection by the Sōgo god spirit for good fortune and victory" ("Sōgo jinrei kaiun shōri go shugo").[30] No longer certain of their rulers, peasant leaders unconcerned with the nativist tradition called on their own gods to bring about a just world. For people in search of symbols of power and legitimacy, the tradition of worshiping peasant martyrs took on political overtones during a time of crisis.

Other protest leaders made explicit use of the peasant martyrs as models for protest. Patricia Sippel has pointed out that after the Bushū uprising of 1866, a wealthy peasant wrote a sympathetic chronicle of the event in which he created his own hero, Gizaemon, an upright, powerful peasant, in the tradition of the self-sacrificing peasant martyr. She concluded that the records of the riot contained not a single instance of antigovernment rhetoric, nor did they show a

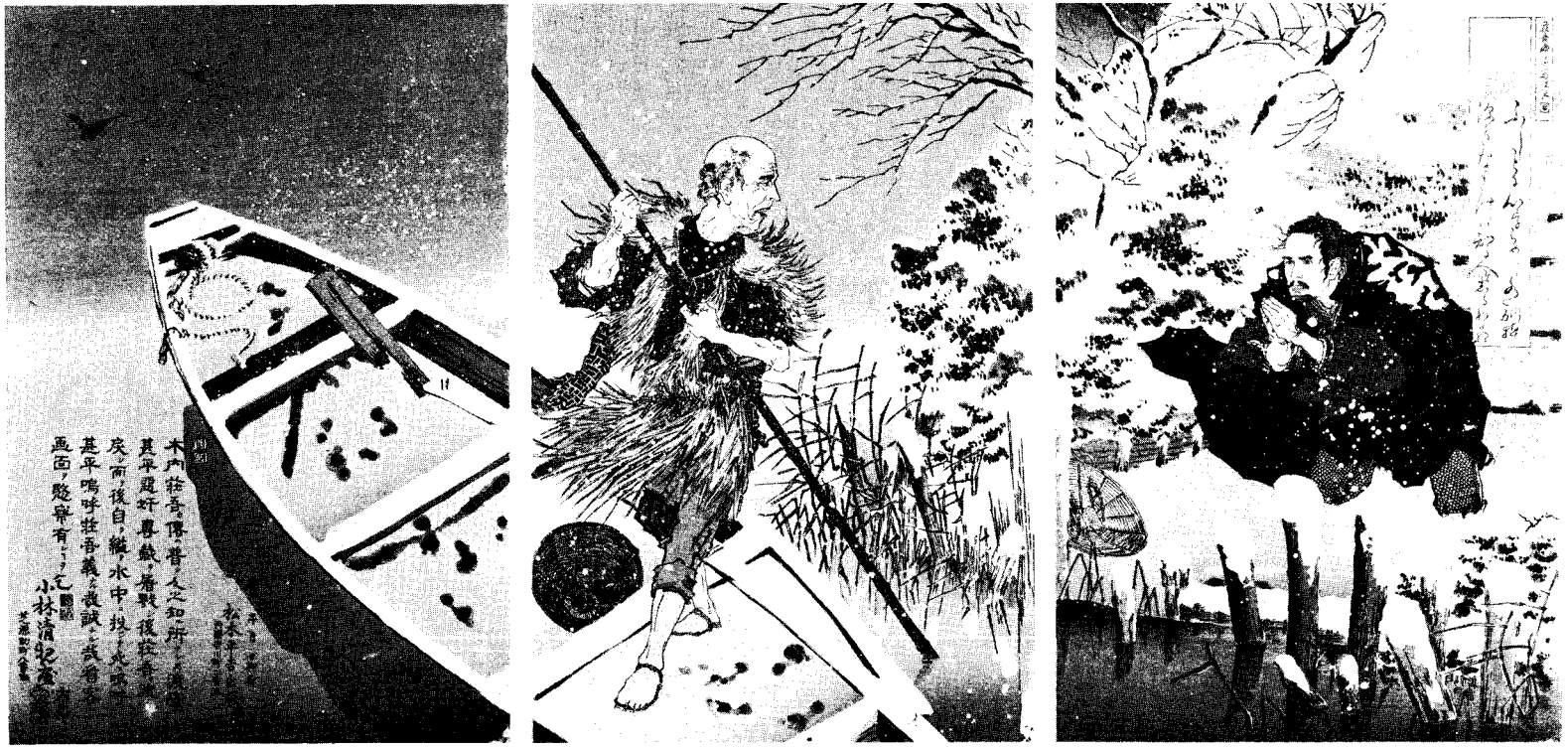
[27] Irwin Scheiner, "The Mindful Peasant: Sketches for a Study of Rebellion," *Journal of Asian Studies*, 32 (1973): 588; Scheiner, "Benevolent Lords and Honorable Peasants," 54–55.

[28] Kujirai Chisato, "Ikki denshō to minshū ishiki no tenkai: Nakasendō Meiwa Tenma sōdō o taizō ni shite," *Rekishi*, 55 (1980): 61–62.

[29] Ōno, *Jizō-dō tsuya monogatari*, 29.

[30] Fukaya Katsumi, *Hachiemon, Heisuke, Bansuke* (Tokyo, 1978), 274–79. In the autumn of 1983, I saw the original woodblocks used to print the talismans at Daigan-ji.

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"Sōgo and Ferry Boat in Snowy Days [Sōgo at the Ferry]." With the noble mien of a nineteenth-century political activist, Sakura Sōgorō bows in gratitude to the wizened boatman Jinbei, who has risked his life by taking the martyr to a last fare well with his family. Woodblock print of a triptych by Kobayashi Kiyochika, 1884. A gift of Carl Holmes to the Los Angeles County Museum of Art, by whose courtesy this photograph appears.

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broader attack on the existing social order.[31] Nevertheless, Gizaemon was clearly modeled after leaders who had placed the interests of their peoples above those of their rulers. In 1864, peasants who opposed being put under the administration of the Yonezawa domain argued that a martyr named Riemon had died in 1666 so that they might become the shogun's peasants. (As the shogun's peasants, they enjoyed more local autonomy, lower taxes, and greater prestige than did peasants in "private," or *daimyō*, domains.) Yonezawa officials destroyed Riemon's grave and memorial tablet, built fifteen feet high and nine feet wide in 1827. In its stead, officials posted a sign: "The vagrant Riemon was punished long ago for his insolence, but absurd stories are still told about him. To have erected a gravestone giving him an illustrious name forbidden by law and then to have built a huge stone monument to him is insolent and disrespectful." Furthermore, officials reported that "during the recent riot, the foolish people worshiped and believed in Riemon more and more, and they always used what he had done as an excuse for writing their petitions."[32] Clearly, Riemon had become a political symbol used to justify opposition to ruling authority.

By the end of the Tokugawa period, representations of the past that centered on peasant martyrs had begun to spread across Japan and gain access to the public field. Sōgorō's tomb became a pilgrimage site, and songs recounting his deeds appeared far away in Shikoku and western Japan. A nursery song from Tokushima went like this: "Sakura Sōgorō/Strapped to a balance beam/O-san, his wife, wept bitterly."[33] Incorporated into the routine of daily life, such songs reminded peasants of the cruel fate suffered by peasant martyrs. In 1851, one of Edo's kabuki theater companies staged a play based on the life of Sōgorō. A lavishly illustrated book about him appeared openly in the streets of Edo a year later. Before Commodore Perry's arrival and the crises of 1853–68, peasant concerns had broken into the field of public representations independent of an exogenous set of stimuli. In 1853, storytellers took up the Sōgorō tale in a dramatization accompanied by the *shamisen*, a shrill, three-stringed instrument. In 1861, a new version appeared in kabuki, followed by the publication of a popular novel.[34]

Scholars have argued that the histories of subordinate groups must break into the field of public representation if they are to become part of dominant memory and endure after the group itself has disappeared or been transformed. Obviously, Sōgorō's fame owes much to efforts by ruling authorities to cast him in the role of a virtuous official. Without this prior recognition, urban authors would not have been able to fashion his story to their own ends. The field of public representation

[31] Patricia Sippel, "Popular Protest in Early Modern Japan: The Bushū Outburst," *Harvard Journal of Asiatic Studies*, 37 (1977): 287–88.

[32] Umezu Hōichi, "Ishibumi wa kataru: aru gimin denshō," in Makabe Jin and Nozoe Kenji, eds., *Minshūshi to shite no Tōhoku* (Tokyo, 1976), 151.

[33] Saigō Nobutsuna, *et al.*, eds., *Nihon min'yōshū* (Tokyo, 1953), 254. For other folk songs, see Bungei Iinkai, ed., *Ri'yōshū* (Tokyo, 1914), 402, and Hayashi Motoi, *Hyakushō ikki no dentō*, 282–89. The most recent publication of the "Sakura kudoki" from Shimane is in Tanigawa Takeshi, ed., *Nihon shomin seikatsu shiryō shūsei*, vol. 24: *Min'yō dowa* (Tokyo, 1979), 603.

[34] Ōno, *Jizō-dō tsuya monogatari*, 29.

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is "crossed by competing constructions of the past, often at war with each other. Dominant memory is produced in the course of this struggle and is always open to contestation."[35] By focusing on a specific means of representing the past—in this case, peasant martyrs—it is possible to use this argument as a historical construct. In Japan, at least, only at times of disorder and crisis did the history created by peasants gain access to the public field. Among the competing visions of leaders of uprisings, as guardians, righteous men, models for protest, cruel victims of fate, or vengeful ghosts, the last was forgotten, and the others that survived into the modern period were reformulated into new representations of history.

THE MEIJI RESTORATION (1868) BROUGHT MANY CHANGES to the tradition of peasant martyrs. The new government pardoned those who had previously been executed as traitors, and peasants were allowed to worship local heroes openly. In 1870, peasants built a new and larger monument to Riemon and added a commendation to inscriptions from the earlier versions of the 1863 riot.[36] But, in other areas, peasant protests and the martyrs who led them were forgotten, their graves and shrines ignored. The festival to Sōgorō, held annually during the Tokugawa period, was discontinued in 1869 when the Hotta domain was abolished.[37] In Shimomuroga village near Ueda, a festival in honor of Isonojō, who died in 1785 for having opposed his headman, stopped taking place. Perhaps worship no longer clandestine no longer seemed important. Similar lapses occurred throughout Japan; for, when political activists later went to the countryside seeking the indigenous roots of democracy, they sometimes had to re-create the old traditions.

Peasant martyrs attracted the attention of scholars searching for Japanese heroes equal to the great democrats of the West. The proponent of the Meiji enlightenment, Fukuzawa Yukichi (1835–1901), praised Sōgorō in "An Encouragement of Learning" ("Gakumon no susume"): "From what I know, there was only one person from ancient times who advocated human rights, who brought pressure to bear upon the government by championing the cause of justice, and who remained steadfast and kept his integrity to the end, when he sacrificed his life. This was Sakura Sōgo, whose biography is found only in popular fiction handed down by the common people, but whose true story has still not been told by historians."[38] Fukuzawa liked Sōgorō—he had acted righteously, but in a nonviolent way. In 1878, a book called *A Biography of Kiuchi Sōgo* (*Kiuchi Sōgo ichidaiki*) was published with the subtitle, *He Bought Freedom with His Death*. Sold

[35] Johnson *et al.*, *Making Histories*, 207.

[36] Umezu, "Ishibumi wa kataru," 160. See also Yokoyama, *Gimin denshō no kenkyū*, 119–22.

[37] Ōno, *Jizō-dō tsuya monogatari*, 31. For evidence of these festivals, see the document written by a warrior official between 1717 and 1723 (in Ōno, 27), domanial records from the 1830s, and temple records spanning the Tokugawa period (Kodama, *Sakura Sōgorō*, 161–76).

[38] David A. Dilworth and Umeyo Hirano, trans., *An Encouragement of Learning by Fukuzawa Yukichi* (Tokyo, 1966), 47. In 1879, Ueki Emori published his "Treatise on Freedom and Popular Rights" with four heroes on the cover: Itagaki Taisuke, Kiuchi Sōgorō, Mori Tōemon, and Fukuzawa Yukichi. In 1875, Kōno Hironaka had praised Mori as a "present day Sōgorō" for his efforts to protect the people of Sakata prefecture. Kelly, *Deference and Defiance*, 226, 257. (Kelly did not explore the ways in which ideologues such as Kōno and Ueki used the symbolism of the *gimin* in the discourse of modern Japanese politics).

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nationally, it praised Sōgorō for understanding the principle of human rights. By praising this peasant martyr, Fukuzawa and his disciples could argue that a notion of freedom existed in Japan before Perry introduced the concept from the West. Therefore, it was not as foreign to the Japanese as the authoritarian leaders of the new Meiji state would have had people believe.

Close to the center of political power, Fukuzawa and his disciples used a specific representation of peasant martyrs as advocates of human rights to contest public constructions of the past promoted by government leaders, constructions in which wise rulers guided and nurtured the "foolish" people. In so doing, the proponents of enlightenment rewrote the history of peasant protest to include only nonviolent, socially conservative action. Fukuzawa and others participated in the creation of the new Meiji hegemony over the field of public history by transforming and including some interests and experiences of the peasants within the ideological contexts determined by the dominant literate elite, while they excluded, suppressed, and socially devalued others—ghost stories, for example. Scholarly proponents of enlightenment broadened notions of what was publicly acknowledged as legitimate—and without legitimation there can be no public action—yet they also excluded the forms of verbally and often physically expressive protest heretofore practiced and recalled by peasants.

In the late 1870s, political reformers went to the countryside to seek and promote their notions of democracy, equality, freedom, and rights. To appeal to peasants' way of thinking, reformers personified these abstractions as peasant martyrs and began to call them *gimin*, or "righteous men," the term by which they are known today.[39] The reformers encouraged local people and local historians to recall their past, erect new monuments to the memories of the leaders of uprisings, search out old records, and rebuild local traditions based on the struggles of Tokugawa peasants against oppression. Theirs was a political purpose, to make ordinary people conscious of their political rights and to make these rights appear to arise naturally out of local history. To this end, reformers encouraged the reinstitution of memorial services and the assumption that these services had been performed continually since before the Meiji Restoration.

Matsuzawa Kyūsuke was a prominent political reformer, born in Nagano prefecture in 1855. In 1879, he became editor of the newspaper *Matsumoto shinbun* and used it as a forum to debate his cause. Between 1878 and 1879, he published serially the book on Kasuke, leader of the 1686 uprising near Matsumoto. Later sold separately, it was entitled *Precepts for People's Rights—A Portrait Of Kasuke* (*Minken kagami Kasuke no omokage*). By 1880, Matsuzawa was organizing popular rights advocates in southern Nagano prefecture and, together with Kōno Hironaka (1849–1923), Tanaka Shōzō (1841–1939), and local leaders of the movement, participated in the alliance to establish a national assembly. He died in prison in 1887. He and others formed a committee to raise money for a large monument

[39] *Gimin* as popular symbols available for a variety of purposes perform a role analogous to that of Marianne in France. In a study of statues representing French notions of freedom and the Republic from 1798 to 1880, Maurice Agulhon pointed out that popular modes of thought characteristically personify abstractions. *Marianne into Battle*, 112.

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inscribed with a long narrative of what Kasuke had done. Since the text was written in archaic Chinese-style writing (*kanbun*), one wonders how many people could read it, something these late nineteenth-century intellectuals evidently did not consider in their efforts to spread enlightenment among the barely literate. The monument was dedicated with a festival held on the 200th anniversary of Kasuke's death.[40] Both festival and monument proclaimed that peasants had acted in the past to oppose the government and improve their lives, emphasizing the point that not just members of the ruling elite but also ordinary people could act politically.

Advocates of popular rights living in the area around Kyoto also tried to enhance awareness of the deeds of peasant martyrs. In Miyatsu in 1879, a play was performed about Kurihara Hyakusuke, leader of an uprising in 1822. A year later, a festival was held in Fushimi to honor Monju Kusuke and other men who, in the 1780s, had rid the city of a corrupt magistrate (*machi būgyō*). To promote the festival, a newspaper, the *Kyoto nichinichi shinbun*, published a biography of Kusuke, written by Komuro Shinsuke.[41]

Komuro Shinsuke (1852–85) became the national leader of the movement to commemorate local peasant martyrs. Between 1881 and 1882, he traveled across Japan, promoting the Liberal party (Jiyū-tō), writing articles for newspapers, and joining festivals and ceremonies. In early 1883, he visited Hashimoto in Wakayama prefecture, where the local branch of the Political Party to Establish a Constitution (Rikken seitō) held a Friends of Freedom meeting. Local reformers gave speeches praising Toya Shin'emon, executed in 1722. One man explained that Shin'emon's deeds in forcing the domanial authorities from the temples of Mount Kōya to reduce the land tax were a direct reflection of the principles of liberty and human rights. In the Meiji period, these same principles appeared in demands for reducing the land tax and establishing an elected assembly. Although local activists tried to amplify and generalize experiences subordinated in the private memories of villagers, their efforts inadvertently came to constitute a liberal reflection of the state ideology that weighed more heavily on private historians after the creation of the Meiji state. The social production of memory served to bolster demands for a constitution. Komuro later gave the biography of Shin'emon first place in his collected stories about peasant martyrs.[42]

In the autumn of 1883, Komuro published the first volume of *One Hundred Biographies of Oriental Advocates of People's Rights* (*Tōyō minken hyakkaden*). He also issued an appeal in the *Jiyū shinbun* for readers to send him materials on peasant martyrs and other righteous men who had sacrificed themselves for the sake of ordinary people. At the end of three volumes, he had collected the histories of

[40] Uejimi Shuichi, *Jōkyō sannen: Gimin Kasuke sōdōki* (Nagano-ken, Minami Azuma-gun, 1975), 85–88. In 1881, Fukazawa Gompachi, from a village in Tama, copied a copy of Sōgorō's biography made by his father and added, "Is 'People's rights' really something new? . . . We can generally cite the peasant rebellions as a beginning of popular rights." Quoted in Irokawa Daikichi, *The Culture of the Meiji Period* (Princeton, N.J., 1985), 133–34.

[41] Komuro Shinsuke, *Tōyō minken hyakkaden*, Hayashi Motoi, ed. (1883; reprint edn., Tokyo, 1957), 385.

[42] Gotō Masato, "Minken-ki Hashimoto chiiki no Toya Shin'emon kenshō to Komuro Shinsuke hen *Tōyō minken hyakkaden*," *Wakayama daigaku kyōiku kiyō jinbunkagaku*, 32 (1983): 15–16.

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forty-six martyrs: two warriors, two priests, one servant, and forty-one peasants. Like the efforts of the pioneering ethnographer, Yanagida Kunio (1875–1962), Komuro's work encouraged local teachers and officials to write down what had been confined to oral tradition. In some cases, local notables argued that, because a peasant from their region had been praised in Komuro's book, it would bring shame on their area if no effort were made to commemorate his deeds with a stone, preferably a large one. Two liberal newspapers reviewed the book, and lecturers on the Liberal party circuit used excerpts from it in their speeches. Popular storytellers recited some of the more picturesque biographies.[43]

In Obama, Komuro's efforts to commemorate the *gimin* met with an immediate response. Two local agitators for governmental reform, Furusawa Shigeru and Shiroyama Seiichi, worked hard to collect legends about Shōzaemon. In 1882, they had a monument erected in his memory. Seven years later, local notables announced plans to erect another monument so that Shōzaemon's deeds would not be forgotten. In a manifesto, they mentioned that Komuro had praised his deeds in the legends of popular rights advocates (*minken gimin*). Donations for the monument were taken from every village and household in the surrounding district. Some people contributed rice, sake, or beans, if they could not afford to give cash.[44] It is difficult to determine to what extent people spontaneously donated their goods and to what extent they participated at the command of a patron. No matter how deeply demands for political reform penetrated Japan's villages, the movement to rediscover local heroes mainly represented the aspirations of local notables, such as landlords, local officials, schoolteachers, and journalists.

The radical, if not revolutionary, wing of the popular freedom movement found its own use for peasant martyrs in politics. On August 17, 1883, Koinuma Kuhachirō, the "fatherly master of the assassination faction," held a memorial service near Utsunomiya in honor of three men executed in 1696 and collectively called the "god of war representative" (Sōdai Hachiman). This was the third mass meeting planned by Koinuma to gather support for the Liberal party. People came from miles around and, according to the mythologizing accounts by contemporary reporters, they filled the plain as far as the eye could see. "Mass meetings have been held from the tip of Kyushu to the depths of Tohoku, but nothing on earth has ever equalled this one." To begin the ceremony, fifteen Shintō priests reverently chanted prayers (*norito*). Then Koinuma advanced to read his speech. At once, police surrounded him and hustled him off the stage. A disciple then read his address to the gods.[45] Koinuma's speech described the deeds of peasant martyrs

[43] Komuro, *Tōyō minken hyakkaden*, 379; Goto, "Minken-ki Hashimoto chiiki no Tova Shin'emon kenshō," 20–21. Trouble with government censors over the characters *minken* ("peoples' rights") in the title led to the second and third volumes being called *Gijin hyakkaden* (*One Hundred Righteous Men*).

[44] Kawamura, *Wakasa no gimin*, 78, 127.

[45] Nojima Kitarō, *Kaba-san jiken*, Hayashi Motoi and Endō Shizuo, eds. (1900; reprint edn., Tokyo, 1966), 29, 60–61. For a description of the Kaba-san incident and Koinuma Kuhachirō, see Roger W. Bowen, *Rebellion and Democracy in Meiji Japan: A Study of Commoners in the Popular Rights Movement* (Berkeley, Calif., 1980), 31–49. Bowen ignored the way activists such as Koinuma used the symbolism of *gimin* in their construction of doctrines of political resistance.

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and the harsh conditions under which they had lived. It ended with a stirring call to action:

> Today in our country the glorious age of imperial rule has been restored, there is much talk of popular rights, and everyone has been won over by the winds of freedom. As a matter of course, these loyal spirits [*shishi*] who opposed the oppression of military government are now revered . . . If we did not manifest their exploits, we would lose the opportunity of performing that duty owed by descendants to their ancestors. Therefore, we are gathered here to record the deeds that granted eternal happiness to the peasants of these ten villages and to make the heroic names of these men known to the world. If people who advocate popular rights make a noisy clamor about freedom and rights, but then begrudge their lives and property, vacillate and hesitate, these men will laugh at us from their graves. The advocates of popular rights in the present must receive inspiration from the deeds of these men. Here we express a small token of our gratitude and encourage those who advocate popular rights today.[46]

To promote his own ideas about freedom, rights, and the necessity for struggle, Koinuma glorified men who had died two hundred years earlier. Unlike Fukuzawa, who praised Sōgorō from his study, Koinuma called for direct action to oppose tyranny. The reaction of the police shows that the government understood full well the implications of Koinuma's speech. The authorities could not afford to ignore the use of peasant martyrs as a symbol for action in the present.

Violent events during 1884 soon led nationally known liberals to renounce their alliance with the peasants. In June, Komuro was sent by his newspaper to cover the Sino-French War, and he did not return to Japan until November, after the Kaba-san incident, in which Koinuma and his confederates had planned to kill high government officials with bombs, and during the Chichibu insurrection, when peasants burned government offices and silk warehouses.[47] Frightened by the fury of the masses, Komuro and other advocates of popular rights stopped talking about indigenous equivalents to Western democrats, and they tried to exclude *gimin* as representations of protest from the constructed field of public relevance. Nevertheless, efforts to commemorate peasant martyrs continued to move villagers long after the leaders had repudiated rural troublemakers. Following the crises of the early 1880s, the tradition of commemorating peasant martyrs continued to evolve.

During the last two decades of the nineteenth century, local notables produced new versions of their regional histories that emphasized their ancestors' defense of their villages and assertion of village rights against a repressive feudal authority. A look beyond the violent incidents (*gekka jiken*) and the posturing of liberals such as Itagaki Taisuke reveals adherents to the popular rights movement throughout Japan, even if they did nothing more than praise ancestors who sacrificed their lives in forcing the ruling authorities to listen to them.[48] The martyrs who were

[46] Nojima, *Kaba-san jiken*, 69–70.

[47] Komuro, *Tōyō minken hyakkaden*, 376, 389, 390.

[48] In 1887, the people in Hamochi district, Sado, erected a monument in honor of Zenbei (d. 1837). A local scholar produced the text. Itō, *Sado gimin den*, 160. In 1886, the putative descendants of Ikeda Saburōzaemon (d. 1611) solicited donations to build a stele in his memory. Aoki Kōji, ed., *Hennen hyakushō ikki shiryō shūsei* (Tokyo, 1978), 53–54. I agree with Neil Waters that the history of local politics

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remembered and the people who led the movements to honor them came from the same social class and had the same goal, motivation, and ideology. By contesting the dominant memory of political action in the past, Meiji rural leaders argued that they ought to have a voice in the political policies that affected their lives. Through their efforts, the notion of *gimin* as a metaphor for local interests became part of the public representation of history but at the cost of excluding all elements of violence from memories of popular protest.

In the late 1880s and 1890s, the political reformers' representation of *gimin* began to be co-opted by government leaders and incorporated into the public construction of the past. Between 1888 and 1889, the shrine to Sōgorō that stands in the city of Narita was given its present form, and his gravestone was refurbished. In 1893, the presidents of both the House of Peers and the House of Representatives promoted a monument erected in his memory. A book published at the same time announced that, since the Imperial Constitution had been promulgated and a national diet convened, it was entirely appropriate to emphasize Japan's history of human rights and respect for courageous people. Sōgorō should be exalted as an advocate of popular rights (or *minkenka*, the term used by Komuro Shinsuke a decade earlier).[49]

Local political activists took what had begun largely as a religious practice and reworked it into a sense of the past with overtly political implications. Monuments, festivals, and shrines erected to peasant martyrs in gratitude for their sacrifices or in fear of angry spirits served as a focal point for asserting an indigenous tradition of struggle for rights and freedom or opposition to central authority. This politicized memory did not, however, completely replace a more private sense of the past generated within local culture. In most cases, the belief that the martyr's spirit had become a protective deity continued to maintain its place in rural religion. Only at odd and brief intervals did political movements sweep up *gimin* for a particular purpose, then drop them when their time had passed. For this reason, perhaps, the symbolic content of *gimin* remained fluid, and the expression of opposition also involved struggles over the meaning, public status, and right to this symbol as an existing tradition.

In the twentieth century, competing constructions of *gimin* were mobilized for a variety of political purposes. Commemorative monuments often emphasized that even peasants might sacrifice themselves for the greater good of the village and the state. Sometimes the deeds of peasant martyrs were called *junkyō* or *junnan* (martyrdom), the same *jun* as in *junshi*, which meant to follow one's lord in death by committing suicide. Right-wing nationalists regarded these concepts as useful in convincing peasant youths to join the army and possibly die for the sake of their community and country. In contrast, leftists, often influenced by the ideals of

in Meiji Japan needs to be rewritten with more attention paid to regions where "nothing happened," but I would shift from his emphasis on cooperation with the state to ask in what ways ideas on popular rights were given a local context. Neil Waters, *Japan's Local Pragmatists: The Transition from Bakumatsu to Meiji in the Kawasaki Region* (Cambridge, 1983), 123–31.

[49] Ueki Naotarō, *Gimin Sōgo den* (1952; reprint edn., Chiba-ken, Narita-shi, 1979), 166–67.

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Taisho democracy or socialism, told stories about *gimin* to emphasize feudal oppression and class struggle. By the 1930s, agrarianists had seized on *gimin* as examples of people who fought the central government to preserve a rural way of life. Some representations of *gimin* did not have an explicit political purpose; they simply reflected a desire by local historians to find their own Sōgorō, produce a local history based on the deeds of local people, reinforce the claims of local uniqueness already present in Shintō beliefs, and, by appealing to an expressionistic popular mentality, give the natives an identity and a source of pride.

Some part of the histories of subordinate groups were incorporated into the ethics textbooks authorized by the ministry of education. Parables for children included stories of diligent peasants such as Ninomiya Sontoku (1787–1856), who studied while carrying firewood down a mountain; honest peasants such as Sōgorō, who gave his all for his people; and patriotic youths such as the bugler Kiguchi Kohei, who died a glorious death for his country.[50] Transformed by the state, martyrs who had once been the heroes of specific localities came to represent the essence of Japan's cultural identity. This classroom history submerged differences of time and place in multiple invocations of the same sense of the historical.

People who constructed a past that represented *gimin* as martyrs for the state strove to gain access to the public field of history. The emphasis on sacrifice for a higher purpose went hand in hand with attempts by local leaders to seek recognition from figures of national renown. Village mayors commissioned Tōyama Mitsuru (1855–1944) and Gondō Seikyō (1868–1938), nationalist supporters of Japan's expansion on the Asian mainland and of agrarianism, respectively, to write commendations of peasant martyrs. In a long narrative, Gondō explained how the peasants had struggled for generations against insensitive government bureaucrats. "The men who martyred themselves [*jungi junnan*] were of extraordinarily high moral repute, and owing to their executions, the misery of the masses rent even heaven itself."[51] The army also allowed its members to be used in promoting a connection between local heroes and the nation: General Araki Sadao (1877–1966), a leading exponent of Japan's holy mission in Asia, wrote the inscription for a monument raised to Toya Shin'emon near Mount Kōya. "Service unto death" was his proclamation.[52]

In other regions, local leaders incorporated the dominant history into the representations of their past. The mainstream of national agrarian thought identified the emperor as the source of all that was good in Japan and farm villages as the repository of virtue, an identification sometimes reflected in notions of *gimin*. In 1933, sixty mayors and civic leaders organized the Society for the Preservation and Praise of the Sado Peasant Martyrs (Sado gimin hosankai) to worship the spirits of the Sado *gimin*, praise their deeds, and explain the unique characteristics of

[50] For the story of Kiguchi the bugler, see Ienaga Saburō, *The Pacific War* (New York, 1978), 24–25.

[51] Nagano-ken, Aoki-mura, and Gondō Seikyō, "Gunnai jungi minjireki," reprinted in *Tsuru-shi rekishi shiryōshū*, 2 (1976): 70–73. A monument erected in 1903 in Yawate village proclaimed that the Sado martyrs had died for their country (*junkoku*). The calligraphy was by Tani Kanjō (1837–1911), who had been a lieutenant-general, a member of the House of Peers, and a minister of agriculture and commerce. Itō, *Sado gimin den*, 1, 60, 160.

[52] Gotō, "Minken-ki Hashimoto chiiki no Toya Shin'emon kenshō," 22.

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The main building of the shrine dedicated to Sakura Sōgorō. Chiba-ken, Narita-shi. Author's photograph.

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thought on the island. Their efforts culminated in a monument erected in 1937 inscribed with the message: "We were born in a sacred age and know not the atrocities of bad government. For this reason, we have a great obligation (*on*) to the emperor, but we must also consider the obligation we owe the *gimin* who sacrificed their lives to save the people.[53] In their praise of local heroes, the members of the Sado society tried to reinforce the bond putatively joining the emperor directly to his people. While reminding the farmers of their duty to the nation, however, the society also insisted on the uniqueness of their own local past, with its heroes similar to the Shintō deities who confer significance on each local tradition. Their brand of agrarian nationalism (*nōhonshugi*) thus contained a tension between the attraction and repulsion of the center subsumed under the identification of emperor and *gimin*. At the same time, by being reworked through a community heritage, the national imperial heritage merged more closely with everyday life.

Persons close to their communities often succeeded in appropriating *gimin* to convey their views of the past, but the appropriation by those farther away did not always work. In 1905, Nishikawa Kōjirō (1876–1940), a member of the nascent socialist movement, wrote an article for the *Commoner's Newspaper* (*Shūkei heimin shinbun*) in which he praised Sōgorō for his efforts on behalf of the peasants.[54] Twenty years later, Fujimori Seikichi (1892–1977), an early follower of the anarchist Ōsugi Sakae (1885–1923), published a drama based on the life of Haritsuke Mozaemon.[55] By this time, Mozaemon, like Sōgorō, had become a nationally recognized peasant martyr, a symbol that those who lived in urban centers could use to propagate their own views of the sufferings experienced by ordinary people under a repressive system of feudal exploitation. Cleansed of any taint of localism, Sōgorō and Mozaemon became universal symbols of the struggle waged against ruling-class exploitation, but this vision of their role had little significance for the people who claimed to be their descendants. Nishikawa and Fujimori were political activists from Osaka and Tokyo, and, in general, their ideas were not popular in villages.[56] When local people wrote about their heroes in the 1930s, they often repudiated the policies of the central government and the ideological stance of urban intellectuals. Concerned about the destitution of farm villages and angry that the government did little to help the farmers, they mobilized a sense of their own history to call for action by new heroes, second Sōgorōs, who would rectify governmental policies and save the people.

[53] Throughout the 1930s, plays based on oral legends and anecdotes about the Sado *gimin* were performed across the island. Itō, *Sado gimin den*, 135, 161, 165.

[54] Aoki Michio, "Bakuhan seishi kenkyū to hyakushō ikki kenkyū" *Kōza ikki*, vol. 1: *Ikki shi nyūmon* (Tokyo, 1978), 244.

[55] *Konsaisu jinmei jiten* (Nihon hen) (Tokyo, 1976), 961. For an analysis of the anarchist movement in Japan, see Thomas A. Stanley, *Ōsugi Sakae, Anarchist in Taisho Japan: The Creativity of the Ego* (Cambridge, Mass., 1982).

[56] One exception appears to come from the former Zeze domain near Lake Biwa, where villagers erected a new monument to two brothers in 1919. The vocabulary used to describe the peasants, the government, and the deeds of the brothers is suggestive of Taisho democracy. Aoki Kōji, *Hennen hyakushō ikki shiryō shūsei*, 1: 64–65.

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A desire to procure a true account of local history and a search for a viable model of political action led village mayor and member of the Fukui Prefectural Assembly, Kawamura Jin'emon, to publish his study of Shōzaemon, crucified in Obama in 1652. In a series of articles in the *Wakasa shinbun* in the early 1930s, he criticized the authorities and professional historians who lauded the exploits of the domain's first lord, Sakai Tadakatsu (1587–1662) while ignoring the sufferings of the peasants that made them possible. "But what can you expect from historians when you consider that they receive stipends from the government. Expecting honest judgments from them is like looking for fish in trees." Kawamura argued that Tadakatsu deliberately set out to build a reputation as an enlightened ruler, a project that required great sums of money: "From time immemorial in all countries, central authorities have plundered the prosperity of local farmers." The so-called benevolent Tadakatsu would have ruined the peasants to enhance his standing in Edo had not Shōzaemon called the first peasant assembly in Wakasa history, presented his demands, and forced Tadakatsu to attend to local affairs.[57]

Kawamura created his own representation of the past, not to argue for a tradition of rights but to lament harsh government. Throughout his narrative, he expressed a lively sympathy for the misery of the peasants. His heroes were the farmers' heroes, and he wished farmers to remember that Shōzaemon was one of theirs, a village headman who had acted alone against ruling authorities to save his people. Like most popular rights advocates fifty years earlier, Kawamura chose not to preach revolution or even mass movement. Instead, he recalled an extraordinary individual who had combated the insensitivity of the central government on behalf of agrarian interests. A former politician himself, Kawamura was probably well aware of how ineffective local political institutions had become and how little hope there was of making institutional reforms that would change the farmers' lives for the better. To Kawamura, only a righteous man had any chance of saving the people without creating social disorder.

By the 1930s, collective constructions of tradition had resulted in monuments, memorial services, plays, and the publication of records centering on the deeds of peasant martyrs. It became easier for a Japanese citizen to absorb a consciousness of important figures in local history, but the messages purveyed by these media were varied and conflicting. Ranging from a connection of farmer and emperor to an assertion of rural interests against the central government and an advocacy of class struggle, statements on the significance of *gimin* were calculated to appeal to the contemporary concerns of their audiences. Underlying these diverse trends, however, was a desire on the part of rural inhabitants to find something in their local tradition of which they could be proud, to find some way of distinguishing their locality from that of others, and to create a past unique to themselves. This movement for a community-based history has dominated postwar representations of peasant martyrs.

[57] Kawamura, *Wakasa no gimin*, 5–41. Just before Kawamura published his article, the Obama shrine dedicated to the memory of Tadakatsu was raised to the level of a prefectural shrine amid a three-day festival and general rejoicing.

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THE CONNECTION BETWEEN GIMIN AND EMPEROR DISAPPEARED QUICKLY after World War II, but efforts to use *gimin* to enhance local heritage continued. The idea was to foster local allegiance, often in opposition to the symbols and images of the national heritage. Theodore Zeldin has pointed out the same refusal to accept common political ideals in France. Regional diversity is particularly emphasized in Brittany, where saints unrecognized elsewhere give Bretons a regional allegiance.[58] Apart from controversies and arguments over who the *gimin* were and what their actions signified, memorial services have continued to be performed and memorial tablets erected to appease the spirits of those who died unnatural deaths. The twin dimensions of *gimin* as political symbols and as focal points for folk beliefs have survived to the present.

During the Occupation, representations of the past that centered on *gimin* excited controversy and dissent. In an effort to foster democracy in Japanese villages, some American soldiers, their wives and children joined a memorial service to the Shizuoka *gimin*, creating an identification with the conquerors that later embarrassed people in the locality.[59] In 1950, in Matsumoto, seventeen headless skeletons were discovered and deemed the bones of Kasuke and his family, executed in 1686. Since twenty-eight people had died for his indiscretion, a search then had to be launched for the other eleven. These skeletons were uncovered in another burial pit, and the entire lot was reburied in a single mound. Plans were made to build an elaborate shrine, but, because of wrangling among the promoters, no more than a tin shack and a rambling wooden structure were built.[60] In 1950, Yashiro villagers moved the monument erected in memory of Riemon in 1887 to the elementary school and had schoolchildren participate in a memorial service in his honor. A copper plaque announced that Riemon ought to be revered because he had sacrificed himself for his people. Critics attacked this kind of history making because it used a legend about *gimin* to destroy the private world of individual inclination, exalt the public world of duty to others, and promote the kind of mistaken love of locality that leads to xenophobic nationalism.[61] Critics questioned whether the exaltation of peasant martyrs promoted democracy or totalitarianism, and they sought a correct way to construct a sense of the past that heightened pride in local history.

One way to invoke a sense of the past without a commitment to any political message is to say as little as possible. In general, postwar monuments are much plainer than their prewar predecessors, although simplicity may also reflect a change in taste or a desire to curtail expenses. Usually, the stone itself carries only a simple inscription. One monument erected in 1954 to the memory of the Ōhara martyrs near Takayama reads: "Monument to the Righteous Men in the Ōhara Riot" ("Ōhara sōdō gimin no hi"). On the back, it lists those executed in 1773.[62]

[58] Theodore Zeldin, *France: 1848–1945*, vol. 2: *Intellect, Taste, and Anxiety* (Oxford, 1977), 3, 55.
[59] Gijin kenhikai, *Gijin Gorōemon* (n.p., 1968), 4–10.
[60] Uejima Shūichi, *Gimin Kasuke sōdōki* (Matsumoto-shi, 1975), 52–57. The buildings are located in Nagano-ken, Matsumoto-shi, Shiroyama.
[61] Umezu, "Ishibumi wa kataru," 161–62.
[62] Gifu-ken, Kokufu-chō, Ankoku-ji.

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Local people who know how and why monuments and shrines were built say that they recall their ancestors' spirit of resistance to tyranny and that such memorials teach children local history. These markers serve as an eternal reminder of events belonging to a single region. The minimal inscriptions themselves, however, give no clue as to why these martyrs are remembered. Perhaps those who erect markers hesitate to make a political statement because they disagree among themselves on what it should be, or perhaps politics is less important to them than is maintaining the traditions that give meaning to life in a village. The reappropriation of local tradition thus becomes an end in itself. Recalling the deeds of *gimin* provides a sense of continuity with the past, a use of symbol on the local level that could be said to parallel the significance of the imperial institution on the national level. A symbol like that of peasant martyrs is fundamentally ambiguous. Where an attempt to construct a historical consciousness of opposition is framed and defined by concepts of the community itself, of its traditions and inheritances, a seemingly liberal struggle for rights conceals a profoundly conservative desire to preserve a rural way of life.

Monuments erected since the 1950s have increasingly become elegant and expensive. In 1963, members of the Society to Exalt the Righteous Men in the Movement to Lower Taxes on the Takasaki Domain (Takasaki Goman koku gennō undō gimin kenshō kai) erected both a long, low slab to mark the place where these men had been executed in 1870 and a concrete pavilion to protect it from the elements. In 1969, the Society to Exalt the Martyr Endō Hyōnai (Gimin Endō Hyōnai Kenshō kai) erected a monument near his grave that described his deeds on the front and on the back listed the names of the contributors and the amounts each had given.[63] In 1980, local business people, primarily those who owned the souvenir stands that lined the road to the Sakura Sōgorō shrine, built a waterfall and pond dedicated to Sōgorō on the 325th anniversary of his death. The marker in front says nothing about what Sōgorō did but in bright red letters announces that 1,600,000 yen was collected for the project. Near the pond stands a large *Jizō*, the guardian deity for children, built two years later. Its finely polished surfaces denote the work of a skilled artisan. These recent monuments are a long way from the rough markers carved anonymously by peasants of the Tokugawa period to placate the souls of their martyrs.

Today, people commemorate peasant martyrs to enhance not only community solidarity but community prestige as well. The emphasis on erecting expensive monuments carved by professionals suggests that the purpose of the marker has changed from a desire to unite a group of people in the worship of their gods to a wish to show the world that even villagers have heroes of whom they can be proud. Yet any notion of struggle is foreign to the impulse that built these monuments. Community leaders seem unconcerned that their attempts to develop a community-based consciousness of tradition may become mere examples of the "people's" humble contribution to the heritage of the nation. This conservatism is hardly surprising; the finer the monument, the more likely it is to have been

[63] Gunma-ken, Misato-mura.

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erected by the wealthy, often conservatives, who want to assert a notion of community with themselves as community leaders. Building monuments to other men, preferably their ancestors, who were also the official and hereditary leaders of their villages, they use local history to buttress their own standing in a solid, stable community.

The erection of monuments is popular. It allows rural people to congratulate themselves on preserving the collective memory of local heroes and to do so by the investment of money instead of time. Annual memorial services, however, have been difficult to sustain. Many have ceased to be performed or are held only sporadically. At the autumn festival in Seki village on the 15th of October, people sing the Hyōnai song and dance in front of his shrine. They tell stories of the protest he led in 1764.[64] The persistence of this tradition is in question, for nowadays only the elderly are willing to dance. In Aoki village, only a few of the once-numerous memorial services to peasant martyrs continue to be performed. Yanagida Kunio wrote that during the services and yearly festivals held at tiny local shrines, "history and religion, community and ancestors, men and the divine come together and give meaning to existence."[65] In the case of *gimin*, this coming together occurs less and less frequently.

Significant anniversaries can still draw crowds. On October 24, 1982, Aoki village held a "gimin festival" to commemorate the 300th anniversary of the death of Masuda Yōhei, a village headman martyred in 1682. Sixty-two people, chosen from among the village administrative office, village assembly, Commerce and Industry Society, and each neighborhood, formed a committee headed by the mayor and raised nine million yen. They ordered a "gimin drum" built especially for the occasion by a manufacturer in Suwa, and fifteen members of the youth group in the Commerce and Industry Society practiced eight months to perform on it. The committee also planned to refurbish the graves of the other *gimin* commemorated in the festival along with Yōhei and publish a pamphlet describing the history of the Aoki *gimin* and the steles erected in their memory. A newspaper article in the *Shinano Mainichi* described the preparations and suggested that the festival would also prove beneficial for tourism.[66]

Tourism is big business in Japan these days. A glance at the posters in a railway station shows that one proven way to attract visitors is to advertise a festival with dancing and drums and perhaps a procession of *daimyō*. Festivals are colorful and exciting. For local residents, festivals bring in money and satisfy a longing for a sense of the past without calling for much thought about its content. These festivals, in fact, promote heritage, not history. They give urban people an excuse to escape to the countryside and borrow the mood and trappings, the food and dances of communities to which they no longer belong.[67] More time for leisure and

[64] Kujirai, "Ikki denshō to minshū ishiki no tenkai," 69–70.
[65] Yanagida Kunio, *The Legends of Tōno*, Ronald A. Morse, trans. (Tokyo, 1975), xxiii.
[66] "Gimin sanbyakunen matsuri o keikaku," *Shinano Mainichi* (March 7, 1982), 3. A description of the festival and the words to the songs that accompanied the drummers is in Yokoyama, *Gimin denshō no kenkyū*, 342–66.
[67] Kohara Yukinari, "Folk without Festivals," *Japan Interpreter*, 8 (Winter 1973): 55–62.

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the opportunities for travel have led to an erosion of regional barriers and a destruction of local characteristics. Just north of Matsumoto, a billboard eighteen feet high and two feet wide beckons train passengers to the Kasuke shrine, a protected historical and cultural treasure. Next door is a shop that sells "gimin cakes," a cube of Western-style vanilla cake covered with white sugar frosting.[68]

Modern communications media homogenize local conceptions of the past while offering them a national hearing. Stories about peasant martyrs occasionally appear on television, and schools or communities put on plays in their honor.[69] Peasant martyrs have also been the subjects of novels by local authors. Oba Yoshiaki, who lives in Kanazawa, wrote *Secret Stories of Kaga Domain: A Tale of Righteous Men from the 1830s* (*Kaga hansei hiwa: Tenpō gimin monogatari*) in 1980. A short preface written by a former mayor states: "The authorities arbitrarily bestowed them with death, but now history has bestowed them with everlasting glory." Another Kanazawa native, Katsuo Kinya, has written five books for children on the themes of peasant uprisings and peasant martyrs.[70] Authors writing for urban commuters have also told tales of *gimin*. In 1965, Minakami Tsutomu, a leading representative of literature for the masses (*kokumin bungaku*), published *Castle* (*Shiro*), a novelization of Shōzaemon and the Obama peasants' struggle against cruel officials in 1650, and it continues to sell today.[71] In 1980, Fujisawa Shūhei published *Peasant Martyrs Run* (*Gimin ga kakeru*). One of Japan's most popular historical novelists, Fujisawa told the story of how, in the early 1840s, the peasants from Shōnai prevented the shogun from transferring their lord (to whom they had already paid taxes and granted loans) to another domain. Both Minakami and Fujizawa report that they first heard stories of *gimin* as children, from their families and neighbors. In middle age, they went back to the legends of their youth for the raw material of their novels. Both authors supplemented their memories with research in historical documents to present vivid accounts of political action taken by ordinary peasants.[72] Their books are sold in pocket editions at train stations. Through their efforts, tales of peasant martyrs have spread among Japan's salaried workers. At a time of rapid depopulation for Japan's farm villages and material affluence for most Japanese, these authors have made peasant martyrs a part of the collective conception of the past along with the samurai, the more familiar representation of Japan's national heritage.

Postwar Japanese continue to call on the tradition of *gimin* for use in their different cultural identities and to contest its meaning. Conservative community leaders claim that *gimin* represent the totality of their village interests. For them,

[68] Nagano-ken, Misato-mura.

[69] The text and a description of one such play are in Awano Mikiko, "Gomon uttae jiken o joen shite," *Tama no ayumi*, 26 (1982): 21.

[70] Katsuo Kinya, *Yama kara koe ga kudatte kuru* (Tokyo, 1980). *Tenpō no hitobito*; *Gokayama gurashi*; *Yuki no hito kuitani*; and *Ansei gonen shichigatsu juichunichi* (Tokyo, 1982).

[71] Minakami Tsutomu, *Shiro: Sado no uzumorebi* (Tokyo, 1975), 7–9. *Shiro* won the twenty-seventh *Bungei Shunjū* readership prize when it was first serialized in 1965.

[72] Fujisawa Shūhei, *Gimin ga kakeru* (Tokyo, 1980), 337–39. The incident in Shōnai has been described in English, though without reference to its historiography, by Kelly, *Deference and Defiance*, 66–104.

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the *gimin* have become a symbol of the kind of community they want to see, a solidarity in which ordinary people act in accordance with the dictates and desires of their betters. In these days of mass communications and the homogenization of Japanese culture, the worship of *gimin* and other local deities provides an important way for participants in yearly memorial services to maintain a sense of regional identity. As pressure from dominant systems of memory-making intensifies, people actively and self-consciously seek local heroes and local gods to help them maintain a sense of place. Authors writing for a general audience tell stories of *gimin* as part of a history centered on peasants or ordinary people, a history that supplements the warrior-centered view of the Japanese found in textbooks and on television. For local writers, stories of *gimin* provide a mode in which to argue local interests against those of the city centers. Activists on the left have often held *gimin* suspect for that very reason. They know that conservatives have been able to appropriate a sense of tradition constructed solely in terms of locality and discard its critical perspective on contemporary social relations. Yet, for opponents of Narita International Airport, Sōgorō is still their ancestor.

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The History of Fundamental Law in Political Thought from the French Wars of Religion to the American Revolution

MARTYN P. THOMPSON

THE HISTORY OF THE TERM "FUNDAMENTAL LAW" from the French Religious Wars to the American Revolution is usually told as a history of constitutional theories. Guided by J. W. Gough, C. H. McIlwain, André Lemaire, and Otto von Gierke, scholars have assumed that "fundamental law" was first coined to denote a theory of constitutional law conceived as the customary law of an ancient constitution. This meaning of the term is again supposed to have been the "normal" meaning until, in the late eighteenth century, it emerged in the narrowly legal and largely American notion focused on the idea of judicial review.[1] On these assumptions, scholars have deemed it appropriate to limit the history of fundamental law to an essentially Anglo-French or Anglo-American affair. These assumptions are distortions of our view of history.

To be sure, the term fundamental law does seem to have first appeared in Theodorus Beza's *Du droit des magistrats* (1573), and it did receive a momentous redefinition in the *Federalist Papers* (1787–88). But the first meaning of the term was not that of ancient, customary, constitutional law. The earliest evidence has been read backwards. The term has been related, exclusively and over-specifically, to one particular constitutional theory. Much that was characteristic of the meaning and significance of arguments about fundamental law in the political discourse of seventeenth and eighteenth-century Europe has been overlooked. "Fundamental law" was a phrase of European as well as transatlantic purchase and needs to be interpreted as such. It gradually acquired accretions of meaning in the course of both practical political argument and theoretical inquiry. Each of its meanings

This paper began as a contribution to the International Colloquium "Assemblee di stati e istituzioni rappresentative nella storia del pensiero politico moderno (XV–XX secolo)" held at the University of Perugia in 1982. I am greatly indebted to the organizers. The present version is a reduction of a much longer version. For comments and helpful suggestions, I am indebted to Maurice Cranston, Harro Höpfl, Ken Minogue, J. G. A. Pocock, J. H. M. Salmon, Mark Goldie, Fritz Kemmler, John Marshall, Paul Westney, Sarah Hoskins, and the editors and anonymous reviewers of the *AHR*. The mistakes that remain are mine.

[1] J. W. Gough, *Fundamental Law in English Constitutional History* (Oxford, 1955), 1–223; Charles Howard McIlwain, *The High Court of Parliament and Its Supremacy* (New Haven, Conn., and London, 1910), 42–108; André Lemaire, *Les lois fondamentales de la monarchie française d'après les théoriciens de l'Ancien Régime* (1907; reprint edn., Geneva, 1975), esp. 136–50, 219–36; and Otto von Gierke, *The Development of Political Theory*, B. Freyd, trans. (New York, 1939), 299–361. For the bold statement of the supposed transition to judicial review, see Gough, *Fundamental Law*, 223.

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was anchored within the linguistic and rhetorical contexts in which the term was first coined, but each, as in all cases of significant language use, was variously intended and variously understood. The term only gradually acquired these associations with specific constitutional theories that historians have mistakenly believed were present all along.[2]

An important new start in a comprehensive history has recently been made by Harro Höpfl in "Fundamental Law and the Constitution in Sixteenth-Century France." Höpfl set out to show, contrary to received opinion, that the idea that France possessed an ancient constitution with fundamental laws was not a significant part of sixteenth-century political thought. He did so by showing, first, that the term *loi fondamentale* was coined late in the sixteenth century, and that, when it became common currency from the late 1580s onward, its meaning was far from unequivocal and certainly not that of basic constitutional law. Second, he showed that it is a mistake to read sixteenth-century references to *anciennes loix*, *leges imperii*, or *loix et observances de la France* (terms often taken to be synonyms for the phrase *loix fondamentales*) as if the writers were drawing on a theory of constitutional law. When French publicists began writing about fundamental law, they were not in possession of modern concepts of either a constitution or a state.[3]

Instead of reading modern preoccupations and presuppositions into the earliest French references to fundamental law, Höpfl suggested that those references be understood as rhetorical devices designed to identify specific rights, privileges, institutions, laws, and habits that various writers, at first Huguenot, then *Ligueur*, thought sufficiently important and sufficiently threatened by princes and magistrates to warrant a special nomenclature, betokening a special defense. The rhetoric proved enormously attractive. In accounting for its appeal, Höpfl referred to the popularity of foundation-edifice metaphors in contemporary political argument. The expression fundamental law, he noted, was "parasitic upon the prior popularity of certain foundation-edifice metaphors." Certainly, such metaphors abound in sixteenth and seventeenth-century European political literature. Among the candidates ("piety," "justice," "religion," and "economic power") that writers suggested were the foundations of their political arrangements, "law" itself was prominent. This form of the foundation-edifice metaphor was most closely proximate and most readily conducive to both the emergence of the term fundamental law and its initial rhetorical appeal.[4]

[2] This critique of conventional histories of constitutional theory and the summary of an alternative view have been inspired by the very helpful theoretical articles of Quentin Skinner, J. G. A. Pocock, and Michael Oakeshott. For a criticism of Gierke's writings on contract theory from the same standpoint, see Harro Höpfl and Martyn P. Thompson, "The History of Contract as a Motif in Political Thought," *AHR*, 84 (1979): esp. 919–27.

[3] Harro Höpfl, "Fundamental Law and the Constitution in Sixteenth-Century France," 1–30. Paper delivered at the International Colloquium on "Die Rolle der Juristen bei der Entstehung des modernen Staates," Tübingen, 1980. The collected papers, edited by Roman Schnur, have not yet been published. I am using the original typescript.

[4] Höpfl, "Fundamental Law," 9–11. See also R. Derathé, "Quelques remarques sur l'emploi, la signification et l'histoire du mot état," in M. Cranston and P. Mair, eds., *Langage et politique* (Brussels, 1982), 151–52.

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The habits of thought involved in prevalent foundation-edifice metaphors (rather than imputed traditions of argument from ancient, customary, constitutional law) constitute for Höpfl the linguistic and rhetorical context of common usage out of which the term fundamental law emerged. Höpfl overlooked a second metaphor in his concern with the rhetorical purchase of the adjective fundamental, namely, that laws were in some sense contracts. This second metaphor is as essential as the first for understanding the manner in which subsequent distinctions between fundamental and non-fundamental laws came to be theorized. It also explains in part the appearance of terms such as "fundamental contracts," "fundamental constitutions," and *pacta fundamentalia* in European political discourse. By the sixteenth century, it is common to encounter the law-contract metaphor in practical political argument. No doubt, the metaphor was popular because the political and legal practices of the loosely consolidated European states suggested contractualist ideas. But a law-contract metaphor could claim a tradition of scholarly speculation stretching back at least to Cino of Pistoia in the early fourteenth century.[5]

Both metaphors were central in Beza's argument in *Du droit des magistrats,* the work that contains the first reference to *loix fondamentales* that I have been able to find. Beza's purposes, of course, were decidedly practical: to provide Huguenots with a theory justifying resistance in the immediate aftermath of the Massacre on Saint Bartholomew's Night (1572). The meaning of this new term was loose, but clearly different from subsequent meanings. As a good Calvinist, Beza limited the right of resistance to "lesser magistrates." The Estates might punish tyrants; lesser magistrates might resist them; but private citizens, unless they had a "special calling from God," must be patient and pray.[6] The argument rested on an interpretation of contemporary French political arrangements. Sovereignty was supposedly divided between monarch, Estates, and lesser magistrates. The relations between rulers and ruled were ones of mutual obligation. In making his case, Beza appealed to the actions of the "fondateurs de la monarchie," the laws and conditions on which "l'estat public est fondé" and the obligation "qui a esté contractee," he supposed, by common and public consent. In all this, he revealed his commitment to both foundation-edifice and law-contract metaphors. Within the immediate context of his discussion of founders, foundations, laws, and contracts, Beza began to talk about the "loix fondamentales d'un Royaume."[7]

The French, he suggested, had originally established an elected monarchy with the Estates empowered to depose kings, should the need ever arise. In time,

[5] Höpfl and Thompson, "History of Contract," 928–30 and n. 18; R. W. Carlyle and A. J. Carlyle, *A History of Medieval Political Theory in the West,* 6 vols. (Edinburgh and London, 1970), 6: 13–23, 153–57; J. W. Gough, *The Social Contract* (2d edn., Oxford, 1957), 47; Otto von Gierke, *Political Theories of the Middle Age,* F. W. Maitland, trans. (Cambridge, 1912), 80–81, 181.

[6] I have consulted the following editions of Beza's *Du droit des magistrats*: that contained in Simon Goulart, ed., *Mémoires de l'Estat de France, sous Charles Neufiesme,* 3 vols. ("Meidelbourg," 1578), 2: 735–90; the partial translation in J. H. Franklin, ed., *Constitutionalism and Resistance in the Sixteenth Century* (New York, 1969), 97–135; and R. Kingdon, ed., *Theodore de Bèze: Du droit des magistrats* (Geneva, 1970).

[7] Kingdon, *Theodore de Bèze,* 24, 55, 61; and Goulart, *Mémoires,* 2: 492.

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however, the Estates had degenerated into the instrument of a few individuals. "Such practices," Beza argued, "are completely contrary to the good old customs of our ancestors and directly contradict the fundamental laws of the French kingdom established at the time of its foundation." Records and reason affirmed that sovereign powers enjoyed only a limited and conditional obedience. If sovereigns departed from the "good laws and conditions" of the kingdom, or if they refused to take "good advice," lesser magistrates and the Estates had a "sworn duty to God and to their country" to resist and punish them. The duty arose, according to Beza, from the contractual nature of oaths of office and was justified by the rules of equity and natural law.[8]

When it was first used, then, the term fundamental law meant something rather loose but reasonably clear. Fundamental law was ancient law established at the foundation of the monarchy. The foundation was a contract. The obligation to keep fundamental law was contractually incurred and rested on equity and natural law. A looseness of meaning arose because of all the possible conceptual inquiries, which Beza did not enter into. He had nothing to say about the relationship between fundamental and other ancient laws or customs, about the content of fundamental as opposed to non-fundamental law, or about the relationship between fundamental and natural, or divine, law. It was not his purpose to address these questions. The term fundamental law seems rather to have suggested itself to Beza in the context of his discussion of foundations, founders, and what he considered the "good old" laws and practices of France. He did not specify his meaning further. His point was to justify Huguenot resistance, not to examine the conceptual complexities of either the concept of law or the relationships between laws and pacts.

Over the next few decades, many French writers picked up the term, employed it for their own purposes, and endowed it with more specific meanings. Some definitions were incompatible with others. Innocent Gentillet was one of the first to clarify the phrase. His arguments in the *Anti-Machiavel* (1576) and *Briève remonstrance* (1576) found frequent echoes in seventeenth-century discussions of fundamental law. Subsequent readers found Gentillet's elaboration of the foundation-edifice metaphor and his attempts to distinguish between fundamental and non-fundamental laws particularly important. Fundamental laws, according to Gentillet, were the columns on which the edifice of king and kingdom was erected. The point of this strange imagery (a kingdom as a house on stilts) was to emphasize that, no matter how grand the edifice of state might appear, it was still very precarious, very vulnerable to attacks on its foundations.[9]

In distinguishing between fundamental and other laws, Gentillet's arguments did not draw on concepts of constitutional versus non-constitutional law. To be sure, he did argue that "if the *fundamental laws* of a kingdom are demolished, the kingdom, the king and the monarchy which are constructed upon them will also

[8] Franklin, *Constitutionalism*, 124.

[9] Innocent Gentillet, *Anti-Machiavel*, C. E. Rathé, ed. (1576; reprint edn., Geneva, 1968), 74, 84; Lemaire, *Les lois fondamentales*, 107–08.

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fall in due course." He added that it "is certainly true that there are certain laws in a kingdom (many even) which may be changed, amended or abolished according to circumstances of time, persons and states of affairs: but the *fundamental laws* may never be changed without the kingdom itself falling very soon afterwards." Also, in the *Anti-Machiavel*, Gentillet insisted that France had three fundamental laws: the Salic law, the three Estates, and the non-alienability of domain. But to interpret Gentillet as groping his way toward a concept of the constitution of a state would be anachronistic. His three fundamental laws were not all laws: they consisted of two ancient laws and one institution. And he was by no means committed to just three. In the *Briève remonstrance*, for example, he greatly expanded their scope. His fundamental laws came to include recent as well as ancient laws, edicts as well as customs, and their content extended deep into the sphere of *droit privé*.[10] The apparent contradiction disappears if we understand Gentillet to be expanding the area of untouchable rights and privileges (in a manner similar to Beza) to include laws, rules, local habits and customs that Gentillet felt were being threatened from above but which were of vital importance for the well-being of France.

Given the conceptual looseness of the earliest references to fundamental laws, it is hardly surprising that this novel terminology was often understood as merely a matter of new words for older and more familiar concepts. In the 1580s, for example, Guillaume Du Vair used the term fundamental law as equivalent to *loix du Royaume*. In doing this, he drew on longstanding distinctions between the laws of a kingdom and those of a king, distinctions that had very recently found formidable expression in the *Instructions des gens des trois estats* (1577) and the famous *Harangue* by President Achille de Harlay in 1586.[11] Pierre Grégoire in 1578 used the Latin *leges fundamentales* in the sense of Jean Bodin's older *lois royales*, a term that Bodin himself had translated with the conventional *leges imperii*.[12] Even Gentillet's extraordinary elaboration of the foundation-edifice metaphor was sometimes read as expressing much older notions. An instructive example is the printer's mark designed for the first Geneva edition of *Anti-Machiavel*.[13] The designer portrayed Gentillet's three pillars as the three traditional foundations of a state (counsel, piety, and *police*), rather than as the three fundamental laws of Gentillet's text. Gentillet's new meaning was thus lost in older, more conventional

[10] Innocent Gentillet, *Briève remonstrance à la noblesse de France* (n.p., 1576), 13–14; Gentillet, *Anti-Machiavel*, 74. Translations from texts are mine unless otherwise indicated. Höpfl, "Fundamental Law," 14, stresses Gentillet's expansion of his fundamental laws or "loix politiques." Gentillet, *Briève remonstrance*, 26–37.

[11] Guillaume Du Vair, *Actions et traictez oratoires*, René Radouant, ed. (Paris, 1911), 110–44; *Instructions des gens des trois estats* in Louis Gonzaga, Duc de Nevers, *Mémoires, 1574–1610*, 2 vols. (Paris, 1625), 1: 448; and "Harangue du Premier Président Achille de Harlay," in Du Vair, *Actions et traictez*, 222–23.

[12] Höpfl, "Fundamental Law," 6.

[13] The mark, illustrated in Figure 1, is discussed in E. Droz, "Fausses adresses typographiques," *Bibliothèque d'Humanisme et Renaissance*, 23 (1961), 574–83, and in A. d'Andrea and P. D. Stewart, eds., *Innocent Gentillet: Discours contre Machiavel: A New Edition* (Florence, 1974), xxi. Of the ten editions that appeared by 1580, seven contained at least one version of the device and motto.

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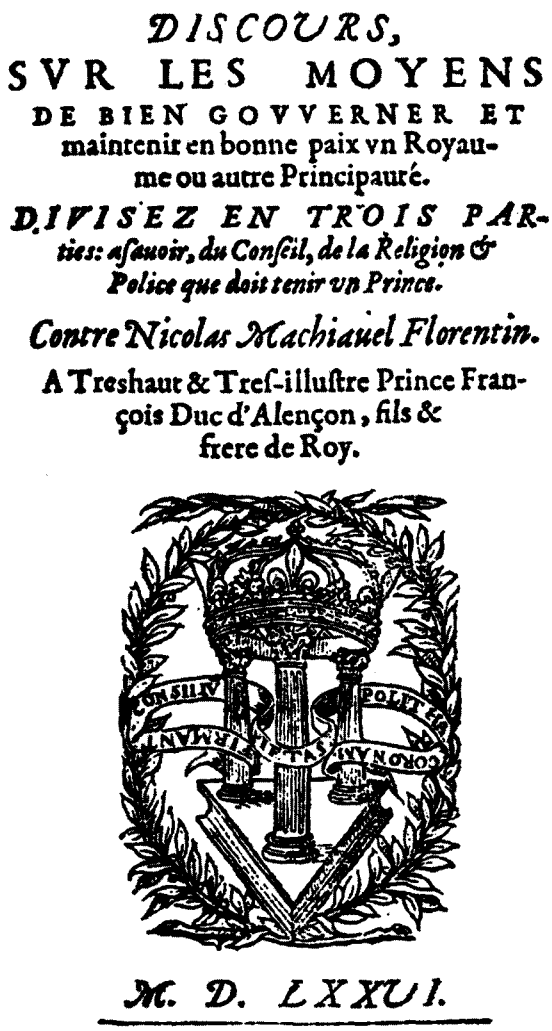


FIGURE 1: Printer's mark from Innocent Gentillet's *Anti-Machiavel* (Geneva, 1576). The book discussed three *lois fondamentales* (the Salic law, the non-alienability of domain, and the three Estates), depicting them as three pillars supporting the crown and kingdom of France. The printer conventionalized Gentillet's pillars by labeling them: advice, piety, and *police*. See note 13.

notions, although the misinterpretation was encouraged by Gentillet's tripartite division of his book. Similar misunderstandings occurred in translation. Georg Schwarz, for example, in his German translation of *Anti-Machiavel* (1580), rendered Gentillet's novel language of *lois fondamentales* with the traditional German *Gesetz* and *Grundsatzung*.[14] As these examples indicate, not all of Beza's and Gentillet's first readers were impressed by their innovations in political terminology.

[14] Innocent Gentillet, *Regentenkunst oder Fürstenspiegel*, Georg Schwarz, trans. (Frankfurt am Main, 1580), 45v. Du Vair, *Actions et traictez*, 126, 128–29, 144.

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As various publicists and translators were assimilating the language of fundamental law into more traditional modes of legal and political thought, others were striking out in new directions. In several pamphlets of the League and the proceedings of the Estates Generals of 1588 and 1593, an idea of fundamental law emerged as positive law, concerned with the framework of government, distinct from ancient customs, and requiring special procedures for its enactment and change. This new meaning emerged in a practical context. With the death of Anjou in 1584, a Protestant, Henry of Navarre, became heir to the throne. Catholic fortunes looked bleak. The intrigues and murders that followed, culminating in the assassination of Henry III, so alienated a section of the League from their traditional institutions that they began demanding a new civil order for France. An important element in League policy from the mid-1580s to the early 1590s was the attempt to exclude Henry of Navarre from the throne and, when this had failed, to declare his title void. The policy rested on appeals to a supposed fundamental law that French monarchs must be Catholic. The first mention of this law in a League context was at the Estates General in Blois in 1577, where it was declared an ancient tradition. But, in an edict of July 1588, Henry III "willed, decreed and ordained" that the law of Catholicity "be held for inviolable and fundamental law" in France. The king, it appeared, was claiming that he could *make* fundamental laws. The Estates General, meeting shortly afterward, did not object in principle. They only insisted that such laws should be made by king and Estates together. This idea that fundamental laws were not necessarily ancient or customary but could be enacted through recognized procedures was shared by the authors of a pamphlet addressed to the Estates General in 1588, called *Articles pour proposer aux estatz et faire passer en loy fondamentalle du Royaume.*[15]

This understanding of fundamental law that some League writers put forward contradicted widely held notions that rejected innovation in law-making and insisted instead that legislation was properly a matter of renovation only, not innovation.[16] Du Vair's famous speech against the Spanish party in the League Estates of 1593 contained an argument about fundamental law that was inspired by traditionalist *renovatio*. The question at issue was the claim of Philip II's daughter to the French throne, a claim that involved breaking the Salic law. Du Vair was unequivocally opposed. The Salic law was fundamental law, and, as such, it could neither be changed nor amended by any legal procedures or treaties. Fundamental law to Du Vair was customary law, ancient and unchangeable. With this conjunction of notions, Du Vair linked the language of fundamental law with

[15] J. H. M. Salmon discusses the circumstances in *Society in Crisis: France in the Sixteenth Century* (London and Tonbridge, 1975), 234–75. Lemaire, *Les lois fondamentales*, 133–42, and Frederic J. Baumgartner, *Radical Reactionaries: The Political Thought of the French Catholic League* (Geneva, 1975), 58–59, 83–199, discuss various understandings of fundamental law. The *Instructions des gens des trois estats* (Gonzaga, *Memoires*, 1: 448) makes a distinction between an ancient custom and a fundamental law precisely with respect to the law of Catholicity. Henry IV complained of the Catholic Estates, especially the Spanish party, in exactly these terms. See *Lettre du Roi a Messieurs des Eglises Reformees, du 25 mai 1593* in André Stegmann, ed., *Edits des Guerres de Religion* (Paris, 1979), 232. I have been unable to consult a copy of the *Articles* but the title makes the point.

[16] The importance of the context of *renovatio* versus *innovatio* is stressed in Salmon, *Society in Crisis*, 14–15.

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traditionalist ideas of French customary law that were being argued by Achille de Harlay and François Hotman, ideas that they traced back to Jean de Terre Rouge in the early fifteenth century. The anonymous author of *Dialogue du Royaume* (1589) attempted to have matters both ways. This pamphlet began by denying that antiquity was essential for fundamental law and went on to advocate *innovatio*, a new start for France with new fundamental laws. But it ended by claiming that a new contract would merely repair the damage inflicted on the French state by internal conflicts; it would be a renovation, a restoration of France's original fundamental laws. Radical League writers such as Jean Boucher pushed the concept of fundamental law as enacted law much further. For Boucher, especially in the *Sermons de la simulée conversion* (1594), what the Estates made, they could unmake. This argument for something like the sovereign authority of the Estates raised the question (though Boucher did not answer it) of whether it made sense to talk of laws as fundamental at all.[17]

A survey of the first French usages of the term *loi fondamentale* makes clear that these many different understandings coexisted. The idea that fundamental laws were inherited laws existed alongside the idea that they were made and makable through recognized procedures. There was certainly no agreement about their scope or about what made them fundamental. There seems no reason today to regard any one meaning as more proper than any other. Each was simply different. The term fundamental law proved immensely attractive to European writers of all political persuasions. By the early seventeenth century, it had become the standard term for any laws, rights, privileges, or customs that writers thought of special importance for the well-being of a community.[18] This standardization was not linguistic debasement. It was simply the continued conceptual looseness that had surrounded the term since the 1570s. Fundamental law became the subject of scholarly inquiry in political theory, jurisprudence, and history. Conceptual clarification during the seventeenth and eighteenth centuries required the construction of still further meanings. But the broad linguistic and rhetorical

[17] Du Vair, *Actions et traictez*, 110–44; François Hotman, *Francogallia*, Ralph E. Giesey and J. H. M. Salmon, eds. (Cambridge, 1972), 466. Confusingly, Giesey and Salmon added the heading "The Fundamental Laws" to Hotman's chapter. Hotman never used the term himself nor did Jean de Terre Rouge. *Dialogue du Royaume; Auquel est discourse des vices & vertus des Roys, & de leur Establissement* (Paris, 1589), 37, 93–94, 142. Jean Boucher, *Sermons de la simulée conversion et nullité de la prétendue absolution de Henry de Bourbon à Saint-Denys, le dimanche 25 juillet 1593* (Paris, 1594), 158. See also the discussions in Lemaire, *Les lois fondamentales*, 139–50, and Charles Labitte, *De la démocratie chez les prédicateurs de la ligue* (1841; reprint edn., Geneva, 1971), 192–206.

[18] Höpfl records the first Latin usage in 1578 ("Fundamental Law," 6); the first appearance of the term in English seems to have been in Bacon's epistle dedicatory to a work of 1596 (Francis Bacon, *Works*, 10 vols. [London, 1826], 4: 6); the first Dutch usage may well have been in the Edict of the States General of 26 July 1581 (E. H. Kossman and A. F. Mellink, eds., *Texts concerning the Revolt of the Netherlands* [Cambridge, 1974], 219); and the first German usage is generally agreed to be in Ferdinand III's *Wahlkapitulation* of 1636 (Adolf Laufe, *Rechtsentwicklung in Deutschland* [2d edn., Berlin, 1978], 101). Practically all the authors who have written on the history of fundamental law have assumed that there was a normal meaning and that this was much older than the coining of the term. See, for example, Gough, *Fundamental Law*, vi; Lemaire, *Les lois fondamentales*, 15–16, 142, 149–50; W. F. Church, *Constitutional Thought in Sixteenth-Century France* (Cambridge, Mass., 1941), 82–83; and Baumgartner, *Radical Reactionaries*, 260 n. 42.

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context of common usage from which the term first emerged continued to color future meanings for more than a century and a half.

I have characterized this context in terms of two basic metaphors. Wherever the vocabulary of fundamental law appears, a manner of thinking in which laws are pacts and political arrangements are buildings was always close by. These images, of course, eventually lost their hold on the European and Anglo-American political imaginations. Such images play no part in the modern concepts of fundamental law. But the failure to note their centrality to early modern European concepts has deflected the attention of historians away from the fact that the history of fundamental law was intimately linked with the history of European contract theory and with one particular image of the state as a building. Both metaphors were ambiguous in the same way. The foundations underpinning the edifice of legal and political arrangements and the contracts that established laws could be understood either historically or rationally. In the original context of common usage, fundamental law was both "good old" established laws and habits and recent law. The implicit tension—between whether fundamental law was fundamental because it was ancient and traditional or whether it was fundamental because it was rationally right—became explicit in attempts to theorize the term, attempts that scholars pursued in one of two directions. Those scholars who discussed ancient legal arrangements by reference to law books and the histories of particular polities used a historical mode of argument, while those who elicited the logic of social and political arrangements used a rational mode of argument.

As J. G. A. Pocock has shown, the historical direction was pursued most resolutely in seventeenth-century England. Pocock highlighted the work of Edward Coke in the early part of the century and William Petyt and William Atwood in the later part. But, contrary to Pocock's argument, this type of inquiry, albeit with significant variations, was not confined to England. In France, for example, a similar direction is evident among Claude Joly and the pamphleteers of the Fronde. In the German Empire, where legal thinkers confined themselves almost exclusively to the categories of Roman law, a number of jurist-historians of the seventeenth and eighteenth centuries began asserting the continuity of an ancient German constitution and the concomitant primacy of ancient German customary law over Roman law. Arguments from ancient and continuing fundamental laws encountered increasing historical criticism concerned with the character of historical "foundations" and the relationship between past and present institutions.[19]

The rational direction was pursued most resolutely by the European jurisconsults. It, too, was vulnerable, but not to historical criticism. Rather, the logic of sovereignty seemed to make fundamental law-as-contract at best redundant, at

[19] On Edward Coke, see J. G. A. Pocock, *The Ancient Constitution and the Feudal Law* (Cambridge, 1957), chap. 2, and Gough, *Fundamental Law*, chap. 3. On Petyt and Atwood, see Pocock, *Ancient Constitution*, chaps. 8 and 9. On the Fronde and Claude Joly, as well as Saint-Simon and Boulainvilliers, see Lemaire, *Les lois fondamentales*, 166–75, 198–210. On the *Reichs-Historiker*, such as Nicolai Hieronymous Gundling, Johann Peter von Ludewig, Johann Jacob Moser, Jacob Karl Spener, and Burkhard Gotthelf Struve, see below, Section 3.

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worst self-contradictory. Either positive law was in its nature, as Jean Bodin had asserted, the command of whoever held sovereign power (in which case any special obligation on the sovereign and subjects to adhere to fundamental laws was a problem), or sovereign power was limited and imperfect in some way. It was questionable what fundamental law could achieve that could not be achieved by recourse to more traditional limits of divine law, natural law, prescriptive rights, or *salus populi*.[20] The logic of sovereignty in the writings of Bodin, Francisco Suárez, Hugo Grotius, Thomas Hobbes, Benedict Spinoza, and Jean Jacques Rousseau, for all the other differences between them, left no place for fundamental laws. Writers such as Johannes Althusius, Christopher Besold, Ulrich Huber, Jean Barbeyrac, Jean Jacques Burlamaqui, Christian von Wolff, J. H. G. von Justi, and Gottfried Achenwall, who did find room for contractual fundamental laws, seem rather to have chopped their logic than pursued it in order to accommodate what they considered the valuable constitutional realities of England, Holland, Poland, and, especially, the German Empire.[21]

The two directions of theorizing were rarely pursued in total isolation from one another. Indeed, in seventeenth and eighteenth-century Europe, reason and history were usually seen as different but complementary inquiries. History portrayed what reason taught.[22] An appeal to both history and reason, then, characterizes each direction: the one, to be sure, focusing on history and appealing to reason either to confirm a historical interpretation or to fill a gap where records were silent; the other focusing on reason and looking to history to confirm rational conclusions. In these ways, the line between the two inquiries was frequently crossed, a crossing eased by a shared vocabulary of fundamentals, foundations, and contracts, and by the ambiguities of the metaphors underlying any reference to fundamental laws.

This rich and confusing potential in the language of fundamental law is evident in what seems to be the first attempt to incorporate the term in a scholarly political

[20] For the complexities of *salus populi* in England, see J. A. W. Gunn, *Politics and the Public Interest in the Seventeenth Century* (London, 1969). The appeal to *salus populi* was double edged. In Hotman and the English radicals, it was undoubtedly meant to limit the competence of the supreme magistrate. In Hobbes, on the other hand, it was grounds for a dispensing power in the supreme magistrate.

[21] For the rational direction, see Johannes Althusius, *Politica methodice digesta* (3d edn., Herborn, 1614), esp. chaps. 19–20; Christopher Besold, *De majestate*, in *Operis politici* (Strasbourg, 1626), esp. 5–8; Ulrich Huber, *De jure civitatis* (4th edn., Frankfurt, 1708), esp. book 1, sect. 3, chap. 5; and book 1, sect. 10, chap. 6; Gottlieb Samuel Treuer, *Sam. L. B. de Pufendorf: De officio hominis et civis*, 1 vol. (3d edn., Leipzig and Wolfenbüttel, 1734), esp. book 2, chap. 6; Christian von Wolff, *Institutiones juris naturae et gentium* (Halle, 1750), esp. sects. 984–89; Christian von Wolff, *Jus naturae*, 8 vols. (Frankfurt and Leipzig, 1740–48), 8: esp. part 8, sect. 77; Joachim Georg Daries, *Institutiones jurisprudentiae universalis* (3d edn., Jena, 1748), esp. sects. 785–94; J. H. G. von Justi, *Natur und Wesen der Staaten*, H. G. Scheidemantel, ed. (Mitau, 1771), esp. sects. 45–46; Gottfried Achenwall, *Jus naturae* (Göttingen, 1774), esp. book 2, sects. 109, 112, 148. The difficulties of reconciling fundamental law with Bodinian sovereignty are notorious. See Otto von Gierke, *Natural Law and the Theory of Society 1500 to 1800*, E. Barker, trans., 2 vols. (Cambridge, 1934), 1: sect. 17 and 2: 333–66. It should be noted, however, that Spinoza retains the phrase and Rousseau modifies its meaning. See Benedict Spinoza, *A Political Treatise*, in R. H. M. Elwes, *The Chief Works of Benedict de Spinoza*, 2 vols. (New York, 1951), 1: 356, 383; B. Gagnebin and M. Raymond, eds., *Jean-Jacques Rousseau: Oeuvres complètes*, 4 vols. (Paris, 1959–69), 3: 362, 393, 436, 491.

[22] For a brief survey of such argumentation in late seventeenth-century English political thought, see Martyn P. Thompson, "A Note on 'Reason' and 'History,'" *Political Theory*, 4 (1976): 491–504.

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theory: Althusius' *Politica methodice digesta* (1603). This work provided countless later scholars with a systematic account of the *consociato universalis* in which *lex fundamentalis* had a prominent place. Althusius drew on much of the French literature that I have discussed, and his argument reflected the intermingling of history and reason, foundations and contracts, pillars and pacts that he found there. But the law that he considered fundamental was, unequivocally, positive, constitutional law. After considering the character of simple and private associations and of the mixed and minor public associations of city and province, Althusius turned to the nature of the universal and major public association of the state. It alone possessed sovereignty, and that sovereignty was located in the organized body of the commonwealth. Like each of the lesser associations, the commonwealth was a contractual body. Civil laws defined and guaranteed the rights and duties of its component parts, laws that could be changed by the sovereign body, but only in order that they better conform to divine and natural justice. The sovereign body empowered kings, princes, and "optimates" and could remove them. The institution of Ephors existed for just such a purpose.[23] Thus, the commonwealth possessed sovereignty, the Ephors administered it, and the supreme magistrate exercised a contractually limited, sovereign power. It was in explaining this last that Althusius introduced his notion of fundamental law.

The limits of the supreme magistrate's *contractum mandati*, according to Althusius, were three: "the Decalogue, the fundamental laws of the realm, and those conditions prescribed for [him] in his election."[24] In any given polity, the second of these was the most crucial:

> In the election of the supreme magistrate, the highest concern must be had for the fundamental law of the realm [*lex fundamentalis regnis*]. For under this law the universal association has been constituted in the realm. This law serves as the foundation, so to speak, of the realm and is sustained by the common consent and approval of the members of the realm. By this law all of the members of the realm have been brought together under one head and united in one body. It is indeed called the pillar of the realm.
>
> This fundamental law is nothing other than certain covenants [*pacta*] by which many cities and provinces come together and agree to establish and defend one and the same commonwealth by common work, counsel, and aid. When common consent is withdrawn . . . the commonwealth ceases to exist, unless these laws are rejected and terminated by common consent, and new ones established, without harm to the commonwealth or impairment to its rights of sovereignty.[25]

As with the rest of his argument, Althusius felt obliged to provide evidence for these claims. He did so by appealing to "precepts, examples, and rational evidences"—to the Bible, history, and reason.[26]

The *Politica methodice digesta* contains perhaps the first attempt to theorize three decades of argument about fundamental law and to integrate the notion into a

[23] Althusius, *Politica* (1614 edn.), chap. 18.
[24] Johannes Althusius, *Politica methodice digesta*, F. S. Carney, trans. (London, 1964), 117.
[25] Althusius, *Politica* (1964 edn.), 123.
[26] Althusius, *Politica* (1964 edn.), 117.

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systematic account of the state.[27] Althusius' debt to earlier writers is clear from his copious acknowledgements and from his reworking the common images of foundations as pillars and pacts. His system proved enormously influential in continental Europe and Britain, but its impact was most marked in the various "natural right" schools.[28] Christopher Besold, for example, echoed Althusius' arguments in his *De majestate in genere* (1618). He distinguished between the majesty of the ruler and that of the realm. The majesty of the realm, or "real majesty," was "comprehended in those fundamental laws by which the commonwealth is constituted . . . and on which it rests as on a foundation through common consent." Besold's conclusion was predictable. If the fundamental laws "are removed, the well-being and order of the commonwealth, by which it stands and endures, are necessarily overthrown, or the commonwealth, at least, undergoes mutation, or alteration, into another form." Like Althusius, Besold thought of these fundamental laws as pacts. A fundamental law, he explained, "has the force of a compact and a covenant, for the change of which the agreement of all is necessary."[29] Also like Althusius, he was not only drawing on an immense range of intellectual sources but on the practices of the German Empire, Poland, and the Netherlands as well. These imperfectly integrated states seemed to offer all the historical evidence necessary to warrant talk of ephors, electors, contracts, and constitutional limits.

For all the increasing frequency of appeals to fundamental law in seventeenth-century Europe, it was not inevitable that the great jurisconsults concern themselves with it. Grotius, for example, found no room for fundamental laws, and Hobbes, reflecting on English polemical usages and the systems of continental jurisprudence, found the term fundamental law thoroughly confusing. The logic of sovereignty excluded the kind of constitutional law that Althusius believed provided legal grounds for dismissing magistrates and that Charles I's opponents had used to justify themselves. In *Behemoth* (1682), Hobbes criticized as an "abuse" the Rump Parliament's talk about the "fundamental laws of the nation." One of Hobbes's participants in the dialogue noted coolly: "I understand not how one law can be more fundamental than another, except only the law of nature that binds us all to obey him . . . whom . . . we have promised to obey; nor any other law to a King, but *salus populi*." The other participant asserted that "the only fundamental law in every commonwealth, is to obey the laws from time to time, which he shall make to whom the people have given the supreme power."[30] The reworking of Bodin is evident, and the appeal to *salus populi*, far from limiting the sovereign, provides grounds for overriding established law.

[27] Althusius frequently refers to Lambert Daneau's discussion of fundamental law in *Politices christianae* (1596). I have been unable to obtain a copy. Althusius' and not Daneau's theory was subsequently regarded as an authority in 1603.

[28] It was not confined to those schools. See the impact in England traced in J. H. M. Salmon, *The French Religious Wars in English Political Thought* (Oxford, 1959), 39–57, 87–88, 95, 103–04, 116–17, 121, 130, 159.

[29] Besold, *De majestate*, 6. The translation is J. H. Franklin's from *John Locke and the Theory of Sovereignty* (Cambridge, 1978), 66–67, notes 36 and 38.

[30] W. Molesworth, ed., *The English Works of Thomas Hobbes*, 11 vols. (London, 1839–45), 6: 249, 361.

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Hobbes makes the point less polemically in *Leviathan* (1651). Having discussed various classifications of law, Hobbes added, "There is also another distinction of laws, into *fundamental* and *not fundamental*; but I could never see in any author, what fundamental law signifieth. Nevertheless one may very reasonably distinguish laws in that manner. For a fundamental in every commonwealth is that, which being taken away, the commonwealth faileth, and is utterly destroyed; as a building whose foundation is destroyed." All the advocates of popular sovereignty or double majesty based on fundamental law would have agreed. Hobbes had simply reiterated a version of the foundation-edifice metaphor. But he continued in a direction they would have found unacceptable:

> "And therefore a fundamental law is that, by which subjects are bound to uphold whatsoever power is given to the sovereign . . . , without which the commonwealth cannot stand; such as is the power of war and peace, of judicature, of election of officers, and of doing whatsoever he shall think necessary for the public good. Not fundamental is that, the abrogating whereof, draweth not with it the dissolution of the commonwealth; such as are the laws concerning controversies between subject and subject."[31]

Precisely those things that Hobbes thought fundamental law gave to the sovereign were the things that advocates thought fundamental laws were needed to limit. Hobbes was turning their arguments against them: he was making sense of the language of fundamental law by interpreting the foundation-edifice metaphor in such a way that it was incapable of accommodating the law-contract metaphor. Indeed, in *De Cive* (1647), Hobbes had explicitly criticized all those who "confound *law* and *convenant*," since they overlooked the fact that "contract is a *promise*, law a command," and thus "contracts oblige us; laws tie us fast, being obliged."[32]

Hobbes's reception in England was notoriously negative. On the European mainland, however, his discussion of sovereignty proved controversial but central to all future inquiries on fundamental law. Samuel Pufendorf, undoubtedly the dominant figure in European jurisprudence from the late seventeenth century onward, provides an important example. His attempted synthesis of Grotius and Hobbes allowed no place for the *leges fundamentales* of many of his contemporaries. Grotius, after all, had made nothing of the term, and Hobbes had found it thoroughly confusing. For all Pufendorf's elaborate contractualism and his concern to outline moral restraints on the exercise of sovereign power, he was persuaded by Bodin's concept of sovereignty. He followed Bodin, Grotius, and Hobbes in distinguishing between law and covenant. Despite his insistence on the contractual character of the state, he regarded contractual obligation as moral rather than positive. A succession of otherwise favorable critics, including Daniel Otto, Gottlieb Samuel Treuer, and Gottlieb Gerhard Titius, in their commentaries, annotations, and observations, all noted Pufendorf's "failure" in this respect. They attempted to reintroduce a notion of fundamental law as positive, constitutional

[31] Thomas Hobbes, *Leviathan*, Michael Oakeshott, ed. (Oxford, 1946), 188–89. Compare Sidney on *salus populi* in Hobbes, *Works*, 94, 304, 348, 351, 389, 408, 451.
[32] Molesworth, *English Works of Thomas Hobbes*, 2: 183–85.

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law between the higher moral laws restraining the sovereign and ordinary positive law that was a command of sovereign power.[33]

But, in one chapter of *De jure naturae et gentium* (1672) and in *De statu imperii Germanici* (1667), Pufendorf reflected on the character of limited sovereignty by referring to *leges fundamentales*. His apparently confusing remarks were of enormous consequence. In *De jure naturae et gentium*, for example, Pufendorf began in the manner of Bodin and Hobbes by asserting that the first characteristic of civil sovereignty was "that it is *supreme*." Still following his sources, he went on to argue that, since civil laws issued from the supreme power, this power could not be "directly obligated" by them. But later, in attempting to distance himself from what he took to be Hobbes's extreme conclusions, Pufendorf accepted that there might be such things as limited sovereignties with fundamental laws, even though he still insisted that this in no way impaired the supremacy of the sovereign.[34] This apparent contradiction is the source of Pufendorf's ambiguous legacy to discussions of fundamental law. On the one side, he appeared as an absolutist, on the other, as an advocate of constitutional limitations.[35] Pufendorf was, in fact, more consistent than he has often been taken to be. For the limitations that he admitted might legitimately be placed on supreme sovereigns, the *leges fundamentales*, were always to be understood as the products of pacts, bargains, and conventions. But, precisely as such, they were not, properly speaking, laws. Laws were never to be confused with contracts. Thus, fundamental law, despite the name, was not really law. Other writers developed the point, and it became common in some circles to argue that fundamental laws were not laws *proprie dicta*. In Pufendorf's work, the point remained obscured because he continued to discuss constitutional limitations in the conventional manner as both *pacta* and *leges* without emphasizing that he considered the two terms irreconcilable.[36]

The character of fundamental law was thoroughly unsettled in the late seventeenth and early eighteenth centuries. Fundamental laws were certainly constitutional laws and thus different in content from other positive laws, but their status as laws was unclear. The earliest theorists (Althusius, Jacob Bornitius, and Besold, for example), had insisted that fundamental laws were contracts and had status as both moral and positive laws. When Hobbes and Pufendorf denied the propriety of the law-contract metaphor, they denied a special legal status to fundamental

[33] Samuel Pufendorf, *Elementorum*, W. A. Oldfather, ed., 2 vols. (London, 1931), 2: book 1, def. 13, sect. 2; Gierke, *Natural Law*, 2: 343; Treuer, *Pufendorf: De officio*, 475. On laws and contracts, see Jean Bodin, *Six Books of the Commonwealth*, M. J. Tooley, trans. (Oxford, [1955]), 30–32; Hugo Grotius, *De Jure Belli ac Pacis*, James Brown Scott, ed., 2 vols. (Oxford, 1925), 2: book 2, chap. 14, sects. 9, 13; and Molesworth, *English Works of Thomas Hobbes*, 2: 183–85.

[34] Samuel Pufendorf, *Die Verfassung des deutschen Reiches*, Horst Denzer, trans. (Stuttgart, 1976), 102–07, 124–30; Samuel Pufendorf, *De jure naturae et gentium*, C. H. Oldfather and W. A. Oldfather, eds., 2 vols. (Oxford and London, 1934), 2: book 7, chap. 6, sects. 1, 3, 7–17.

[35] In secondary literature, the first view is expressed in Alfred Dufour, "La souveraineté dans l'Ecole allemande du droit naturel moderne: Pufendorf," in André de Muralt, *et al.*, *Souveraineté et pouvoir*, Cahiers de la Revue de théologie et de philosophie, 2 (Geneva, 1978), 85–111; the second view is approached in Horst Denzer, *Moralphilosophie und Naturrecht bei Samuel Pufendorf: Eine geistes- und wissenschaftsgeschichtliche Untersuchung zur Geburt des Naturrechts aus der Praktischen Philosophie* (Munich, 1972), 160–205.

[36] Pufendorf, *De jure naturae*, 1: book 7, chap. 6, sects. 10, 13, 17.

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laws. Contracts were certainly morally obliging but, as Pufendorf argued, the obligation was "imperfect" until it had been embodied in positive laws.[37] In the following years, writers within the European schools of natural law attempted to mediate between these two positions. These attempts reveal yet different understandings of the character of fundamental law.

A number of absolutists such as Johann Friedrich Horn, Jacques-Bénigne Bossuet, and Justus Henning Boehmer insisted that fundamental laws were pacts and concluded that the obligation to keep them on the part of the sovereign was, indeed, merely moral. It could be overridden by the higher moral obligation of *salus populi*. In this respect, their position was a modification of Hobbes's in *Behemoth*. Fundamental laws were pacts, despite Hobbes, but the obligation on the sovereign was imperfect. Whereas Hobbes had made the law-contract metaphor redundant, these writers retained fundamental law as contract while making the foundation-edifice metaphor redundant. Sovereign will, not fundamental law, was the basis of their state.[38]

Writers such as Ulrich Huber and Christian von Wolff attempted to affirm both a special, positive law status and a peculiar moral force to fundamental law. According to Huber, for example, ruler and ruled were separate bearers of rights, and fundamental laws were pacts between them. Those pacts were ultimately binding according to natural, not positive, law. Drawing on Pufendorf and others, Huber acknowledged that fundamental laws which obliged solely *per modum conventionibus* were not laws *proprie dicta*. He nonetheless proceeded to distinguish between "tacit" fundamental laws (universal moral constraints on sovereigns), and "express" fundamental laws (positive law constraints). The "tacit" category arose from that conditionality which Huber thought implicit in all political arrangements, the "express" group from historically specific express stipulations. In distinguishing between express and tacit fundamentals, Huber combined in the same flow of argument both rational (tacit) and historical (express) constitutional limits to sovereign power. The result was a commitment to divided sovereignty. Wolff's rather different approach ended with the same commitment.[39]

Writers such as J. H. G. von Justi reversed Hobbes's priorities and set the sovereign will of the people above both ruler and constitution. Fundamental laws were positive laws in this view (*Grundverfassungen* and *Grundgesetze* were Justi's terms), changeable with the will of the people. The state originated by contract but the contract was implicit, "stillschweigend." Justi had no time for the complexities

[37] Althusius, *Politica* (1964 edn.), 123; Jacob Bornitius, *De majestate politica* (Leipzig, 1610), chaps. 8–10; Besold, *De majestate*, 6; Pufendorf, *Elementorum*, 2: book 1, def. 12, sects. 15–41, and def. 13, sects. 2–7.

[38] J. F. Horn, *Politicorum pars architectonica* (Utrecht, 1664), book 2, chap. 10, sect. 4; on Bossuet, see Gierke, *Natural Law*, 2: 336, and on Boehmer, 2: 338–40. Hobbes, *Behemoth*, in *Works*, 6: 249, 361; Bossuet, *Politique tirée des propres paroles de l'Ecriture sainte*, Jacques Le Brun, ed. (Geneva, 1967), 25–26, 28–29, 92, 97.

[39] Huber, *De jure civitatis*, book 1, sect. 3, chap. 5, and book 1, sect. 10, chap. 6; Wolff, *Jus naturae*, part 8, chap. 1, sects. 77–78, 120–21. For Wolff on fundamental law, see Bachmann, *Die naturrechtliche Staatslehre*, 175–94. Joachim Georg Daries considers a distinction between *leges fundamentales* and *capitulationes*; the first are limitations in hereditary states, the second in elective ones. Daries, *Institutiones iurisprudentiae universalis* (1746; 3d edn., Jena, 1748), book 5, chap. 3, sects. 791–94.

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of Pufendorf's contractualism. Detailed specification of the processes of right-transference did not accord with the historical evidence of the actual founding of states. But contracts were still crucial to Justi's arguments and central to his confusions. An implicit contract of union was the basis of the state. The people transferred the exercise of sovereign power to a sovereign authority on certain conditions. Those conditions were the fundamental laws. They were directives, however, not contracts. As directives, they could not be changed by any king or magistrate. But Justi proceeded to treat the relations between ruler and ruled as if they were contractual. From the point of view of the ruler, the people ceased to be the source of law and became simply a contractual partner. Sovereignty was divided, and it made no more sense to insist that the fundamental laws were outside the jurisdiction of the supreme magistrate than to say that they were outside the jurisdiction of the people. H. G. Scheidemantel pointed out Justi's contradiction. Justi had set out defending popular sovereignty, but his acceptance of the metaphor that fundamental laws were contracts had led him to a position defending divided sovereignty.[40] His confusion reveals very clearly the difficulties for eighteenth-century jurisconsults who were committed to ideas of popular sovereignty and to the view that laws were in some sense contracts.

So far, I have considered a number of different understandings of fundamental law that arose in the course of European speculation from the early seventeenth to the mid-eighteenth centuries about the logic of sovereignty and its implications for the status of contracts and legal foundations. For all the differences between the views, the potentialities and problems of the contract metaphor were central. But this is only half the story. Further understandings of fundamental law arose in the course of historical inquiries into the origins of European states and the relationships between ancient, fundamental constitutions and present political arrangements. The intellectual threads from which these other concepts were woven were those of the historical mode of theorizing fundamental law, rather than the rational mode of the natural law schools. The historical mode of inquiry was pursued most clearly and consistently in seventeenth-century England.

THE BROAD CHARACTER OF THESE ENGLISH DISCUSSIONS is quite familiar. As J. W. Gough emphasized, the term fundamental law was ill-defined during the seventeenth century. The laws that were considered fundamental varied enormously, from the whole common law to much more limited matters. The term appeared prominently in both parliamentary and royalist literature. It was sometimes equated with natural law, sometimes with ancient, immemorial law. It was sometimes the product of an original contract, sometimes traditional and customary law. The looseness and variety, it would seem, were no less marked in early English usages than in the first French usages. But among the variety, Gough detected a "normal usage," one concerned with denoting "the essential presup-

[40] Justi, *Natur und Wesen*, sects. 17–18, 27–28, 45–47.

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positions of the historic constitution."[41] From his perspective, the normal usage was perverted by the rationalist innovations of the Levellers, and it became submerged during the Commonwealth and Protectorate. It resurfaced at the Restoration, became a battleground for Whig and Tory historians up to 1688, and then gradually lost its relevance, as both the theory and practice of parliamentary sovereignty rendered it redundant. J. G. A. Pocock's study of seventeenth-century legal and historiographical thought has considerably sharpened appreciations of this normal usage: it was Edward Coke's; it arose during Elizabeth I's reign; it identified fundamental law with a body of traditional rights deriving supposedly from time immemorial. What made fundamental law fundamental to the English was precisely its antiquity and character as ancient custom. As a product of the "common-law habit of mind," this was a typically English affair. It was part of an attempt to assert that English law was English custom and uncontaminated by Roman law.[42]

There is ample evidence that this common-law understanding of fundamental law was widespread in seventeenth-century England. But it was not the only understanding. The first English usages were not confined to fundamental law as immemorial, customary law, and the context of usage was never limited to the habits of a common-law mind. When Francis Bacon, for example, first used the term fundamental law in 1596, he stated very clearly that he was referring not to immemorial, customary law but to the specific laws of Edward I. These laws were the foundation on which the English state "ever since hath principally rested." To be sure, more than a decade later, James I noted a tendency in England to equate fundamental law with common law. But his point in doing so was to juxtapose English understandings with very different ones in Scotland, where fundamental law consisted of "those Lawes whereby confusion is auoyded, and [the] Kings descent mainteined." They were part of the "IVS REGIS." In this initial context of usage, contract and building metaphors were as evident as they had been in France. But there was no place for contract metaphors in Coke's notion of immemorial, customary law, and there was very little room for foundation-edifice metaphors, either. Immemoriality rendered irrelevant those ways of thinking that concentrated on historical origins, original contracts, and the pillars of state designed by constitutional architects. But these ways of thinking never disappeared from seventeenth-century English political thought. In attacks on immemoriality and in habits of thinking alternative to those of the common-law mind, there is evidence to suggest that Coke's concept of fundamental law was less typical of seventeenth-century England than it has been taken to be. More familiar, European concepts continued to coexist and interact with it.[43]

[41] Gough, *Fundamental Law*, 160.

[42] Pocock, *Ancient Constitution*, 30–36, 47–55.

[43] Bacon, *Works*, 4: 6; Charles Howard McIlwain, ed., *The Political Works of James I* (New York, 1965), 61–69, 300; Pocock, *Ancient Constitution*, 30–69; and H. Butterfield, *The Englishman and His History* (1944; reprint edn., Hamden, Conn., 1970), 47–62. Part of this interaction in the first half of the seventeenth century is discussed in Francis D. Wormoth, *The Royal Prerogative 1603–1649* (London, 1939), 30–40, and Salmon, *French Religious Wars*, 58–100.

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Richard Tuck has recently argued as much. Coke's understanding of English law as immemorial and unchanging was directly challenged by many of his contemporaries, including William Hakewill and Thomas Egerton, Lord Ellesmere. Indeed, among John Selden and his followers, in Hobbes and the proponents of theories of radical rights from the 1640s onward, more European concepts of law, rights, and government found expression than anything to be read in Coke.[44] Thus, at the same time that Coke and Sir John Davies were dispensing with contract metaphors, for example, Selden was insisting that "every Law is a Contract betwixt the Prince & the people & therefore to bee kept." Custom itself was an implicit contract. During the 1620s and 1630s, as royalists and parliamentarians confronted one another with different interpretations of their ancient and inherited fundamental laws, voices were still raised against English "Puritano-Papistae" who were appealing to foreign notions of fundamental law conceived as a *contractus* or conditional convenant between prince and people. In the 1640s and 1650s, this supposedly foreign school received two of its ablest and most effective champions in Philip Hunton and George Lawson. By that time, the "normal" English usage equating fundamental law with immemorial common law had become relatively rare. Levellers such as Richard Overton and John Lilburne had moved from defending historic rights to arguing from natural rights. Perhaps the most common understanding of fundamental law became that of the terms of an original, constitutional contract.[45]

After the Restoration (1660), common-law understandings of fundamental law gained prominence, but contractualist notions did not disappear. Indeed, from the mid-1680s on, a number of Whig historians, including William Petyt, William Atwood, and James Tyrrell, began searching historical records for evidence of England's constitutional contract and fundamental laws. Their purpose was entirely practical: to find grounds in constitutional law for resisting James II. In common with most of their contemporaries, they connected the excesses of the civil wars with the shift by the Levellers from a concern with concrete and specific traditional rights to the rationalist abstractions of natural rights. Hence, their insistence that James II had broken England's fundamental laws was occasion for more extensive historical inquiry than the similar accusation against Charles I had been. It is surprising to find that Petyt and Atwood were so prominent among the constitutional contractarians, for they began their careers as legal historians in the tradition of Coke. Their initial concern was to evidence historically England's ancient, immemorial constitution. But, in circumstances that Pocock has explored in detail, they changed the focus of their studies. The fundamental laws they detected in Saxon England ceased to be fundamental because they had been enjoyed time-out-of-mind and became fundamental because they were part of the

[44] Richard Tuck, *Natural Rights Theories: Their Origin and Development* (Cambridge, 1979), 82–139, 143–55, esp. 132–39.

[45] Frederick Pollock, ed., *Table Talk of John Selden* (London, 1927), 69, 137; Gough, *Fundamental Law*, 58, 61–64, 76; David Owen, *Anti-Paraeus* (Cambridge, 1622), quoted in Gough, *Fundamental Law*, 228. On Lawson in particular, see J. H. Franklin, *John Locke and the Theory of Sovereignty* (Cambridge, 1978), chap. 3; and Gough, *Fundamental Law*, 117–39.

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original Saxon contract. Despite the magnitude of this change (a prescriptive right is not a contractual right), Atwood, especially, gave every appearance of making it with ease. Part of the explanation, at least, lies in the familiarity of both concepts to the English of the late seventeenth century and in the existence of ready-made bridges between the two concepts, consisting of deceptively similar appeals to equity, reason, and the past, in both common-law habits of thought and the European schools of natural rights.[46]

Outside England, the idea that immemoriality made fundamental law fundamental stood little chance. Prevalent myths about the historical foundations of European states and habits of mind formed by the study of Roman law were hostile to it. In early eighteenth-century France, debates about the character of fundamental law continued in much the same vein as they had since the late sixteenth century. In the constitutional controversies following Louis XIV's death in 1715, three different concepts of fundamental law vied for recognition. Fundamental law, it appeared, was fundamental either because it was willed as such by the king, because it arose from "a primitive and original contract," or because it was simply an "*usage reçu*."[47] The metaphors of law-contract and foundation-edifice were as evident in these discussions as they had ever been. But in one important respect, a change was apparent. Earlier ways of thinking about the character of fundamental law were coming to seem inappropriate for new understandings that were emerging from historical inquiries. Dissatisfaction with the old categories was most clearly experienced in the schools of *Reichs-Historie* that were founded in the German Empire during the late seventeenth and early eighteenth centuries.

Equipped with concepts drawn from Roman law, a number of German jurist-historians—including Nicolai Hieronymous Gundling, Johann Jacob Moser, and Jacob Karl Spener—began asserting the priority of ancient German customary law over imperial Roman law. An important part of their enterprise required proving the continuity of an ancient German constitution. Drawing on Herman Conring's remarkable researches during the mid-seventeenth century, they saw themselves as reforming the study of German public law by supplying the necessary facts to complement and correct those rational abstractions that they believed had dominated legal thinking for far too long.[48] Their commitment to rational abstractions remained. But, in the course of their inquiries, they came to appreciate that their legal categories did not fit their historical evidence. Spener's unfinished *Teutsches Ius Publicum* (1723–33) may serve as an example.

Spener began by noting the confusion in contemporary understandings of the imperial fundamental laws. In Spener's view, they should be seen as laws concerned with the "Formae essentialis" of the Empire. They were variously "Reichs-Constitutionen," "Reichs-Fundamental-Gesetze," and "Reichs-Grund-

[46] Pocock, *Ancient Constitution*, chap. 9, discusses the evidence but makes little of the contractualism.
[47] Lemaire, *Les lois fondamentales*, 197, 201–02.
[48] Johann Jacob Moser, *Compendium Juris Publici* (Tübingen, 1731), 3–52; Nicolai Hieronymous Gundling, *Ausführlicher und vollständiger Discours* (Frankfurt and Leipzig, 1732), prolegomena, esp. 61–62, and part 2, sects. 5 and 6; Jacob Karl Spener, *Teutsches Ius Publicum*, 7 vols. (Frankfurt und Leipzig, 1723–33), 1: book 1, chap. 11, sect. 8, and 1: book 1, chap. 12, sect. 5.

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FIGURE 2: Frontispiece from Jacob Karl Spener's *Teutsches Ius Publicum* (Frankfurt and Leipzig, 1723–33), showing the restoration of genuinely German fundamental laws. The caption reads "Either we rid ourselves of eternal and unknown law, or we injure the Empire." See note 49.

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Gesetze," and they ought to be categorically distinguished from all ordinary "Reichs-Gesetze." But confusion during the seventeenth and eighteenth centuries had been confounded because the principles for interpreting fundamental law had been derived from speculative systems of legal philosophy instead of genuinely German law. The task Spener set himself was to rid the study of imperial public law of all its foreign accretions and to recover the genuinely German legal principles that he believed were embedded in German history. Roman law, Hebraic law, Lombardic law, and the principles of interpretation that were derived from various philosophical texts were all to be dispensed with and replaced by genuinely German law, such as that contained in the various *Reichs-Satzungen*, *Reichs-Handlungen*, the *Golden Bull* (1356), and unwritten customary law.[49] But the German laws needed to be interpreted on genuinely German principles. Spener recommended those principles be derived from a historical study of unwritten, customary fundamental laws—the *Reichs-Herkommen*, as it was called.

Spener drew his idea of the ancient German constitution largely from Tacitus. Like many before him, Spener read into Tacitus an implicit contract, but it served his purposes to make more than was usual of Tacitus' testimony that the earliest public law of the mixed, irregular German state was customary law.[50] The differences between Tacitus' Germany and the contemporary Empire were both obvious and enormous. Spener filled the gap with the following evolutionary history and developing view of fundamental law. When Tacitus wrote, the German state was imperfectly formed. Under the early Carolingians and, especially, under Otto the Great (936–62), it acquired a more coherent shape. Then followed various Roman incursions, and problems arose as to the precise content of the original *Reichs-Herkommen*. Demands were made that it be recorded, and, after initial opposition from the *Stände* under Otto IV (1208–12), they were acted on. The process was continued under Ludwig of Bavaria and eventually resulted in Charles IV's *Golden Bull*. The *Golden Bull* became the imperial equivalent of Magna Carta; it was the "Haupt-Grund-Gesetz" of the imperial constitution.[51] The process of legal evolution did not stop here. The Empire became even more "formed" over the next three centuries until the Peace of Westphalia (1648). This settlement, confirming the *Reichs-Herkommen* and modifying some of the earlier fundamental laws, became now, according to Spener, the "Haupt-Grund" of the imperial *Iuris publici*.[52]

The attack on Roman law, then, resulted in an evolutionary history of the German constitution, rather than, as in England, in an insistence on immemorial and unchanging legal arrangements. Unlike Coke and his followers, Spener and

[49] Unlike Gentillet's printer, Spener's engraver understood perfectly the program being presented in each of the seven volumes completed and published. Each volume has the reproduced engraving as frontispiece. The table of public law has been swept clear of all foreign accretions and filled with the sources of genuinely German fundamental laws, including the principles of natural right embedded in the history of the Empire. Spener, *Teutsches Ius Publicum*, 1: book 1, chap. 4, sect. 8 and note (e); book 1, chap. 4, sect. 8, note (a).

[50] Spener, *Teutsches Ius Publicum*, 2: book 2, chap. 2, and book 1, chap. 2, sect. 3.

[51] Spener, *Teutsches Ius Publicum*, 1: book 1, chap. 2, sect. 3, and book 1, chap. 4, sect. 1.

[52] Spener, *Teutsches Ius Publicum*, 1: book 1, chap. 2, sect. 3.

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the other *Reichs-Historiker* had no doubt that their history could be reconciled somehow with their commitment to notions of contract, foundations, and edifices, notions that they adopted in their speculative legal philosophies. Indeed, the new *Staatslehre* they believed they were establishing was premised on integrating reason and history.[53] But, predictably, they never succeeded in a reconciliation. Their understanding of imperial evolution and their notion of a developing *Reichs-Herkommen* contradicted the contractualist and architectural concepts of law with which they had begun. Thus Spener, having defined fundamental law as created by "State-contracts," felt obliged to warn the readers of his evolutionary history that this was not really the case. Gundling, having insisted that the imperial fundamental laws were the immutable foundation of the edifice of state, felt obliged in the context of his history of imperial law to assert that all human laws were subject to constant mutation. And most disconcerting of all, as Spener realized with obvious embarrassment, it became impossible to reconcile evolutionary fundamental law with the rationalist concepts of obligation that distinguished fundamental from non-fundamental law. Customary law (*Gewohnheitsrecht*) generally was a matter of long usage, and this, according to Spener, imposed no extraordinary obligations. But the customary law of the constitution, the *Reichs-Herkommen*, did impose special obligations as could only arise from contract. Thus, contractual obligation was a distinguishing characteristic of fundamental law, both written and unwritten. But in the evolution of the *Reichs-Herkommen*, there was no evidence of continually changing contractual arrangements. Spener, having made the contractual point about the character of the *Reichs-Herkommen*, felt obliged to qualify it immediately. He knew, he admitted, that it was not really appropriate to talk of customary law as a contract, not even as an "implicit" contract, but he confessed that he could see no alternative way of accounting for its force as fundamental law.[54]

The tensions and doubts involved in the clash of rational and historical methods of interpreting fundamental law remained unresolved. But in the writings of the German *Reichs-Historiker*, those doubts were explicitly centered on the appropriateness of the two metaphors that had been implicit in the vocabulary of fundamental law since the first appearance of the term in late sixteenth-century France. On the one side, the logic of popular sovereignty seemed to make fundamental law-as-contract redundant. On the other, notions of legal evolution strained the plausibility of the foundation-edifice metaphor. It seems, however, that it was not in Germany but in mid-eighteenth-century France that a decisive change occurred. A new understanding of fundamental law arose that had no place for the original metaphors.

[53] Spener, *Teutsches Ius Publicum*, 1: book 1, chap. 11, sect. 8.

[54] Spener, *Teutsches Ius Publicum*, 1: book 1, chap. 1, sect. 2; book 1, chap. 4, sect. 7; book 1, chap. 11, sect. 1; book 1, chap. 11, sect. 2, note (a). Gundling, *Ausführlicher und vollständiger Discours*, 793–94.

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MONTESQUIEU'S ESPRIT DES LOIS (1748) MARKED A NOTABLE DEPARTURE in the definition of fundamental law. For all Montesquieu's interest in the history of European states, he never contemplated searching records for historically evidenced pillars or pacts. His enormously influential discussion of the separation of powers and the English constitution is certainly based on a concept of fundamental law. But the concept does not depend on references to contract or foundation-edifice. Fundamental laws were constitutional laws that "derive directly from the nature of government." They made up the fundamental constitutions of the various kinds of government: democractic, aristocratic, monarchical, even despotic. In other words, fundamental laws were fundamental in relation to types of government, not through antiquity, prescription, reason, or sovereign will.[55]

Because Montesquieu's concept of fundamental law was part of a much broader enterprise, he did not stop to argue this part of his case. Older views still prevailed, and in the *Encyclopédie* (1765) they received one of their most authoritative expressions. The entry of "Loi fondamentale," plagiarized for the most part from Jean Jacques Burlamaqui's posthumous *Principes du droit politique* (1751), clearly shows the continuing hold of the two metaphors and the confusions to which they were giving rise. Fundamental law was defined as constitutional law; it was old, the "primordial law of the constitution of a government." It comprised both "ordonnances" and "conventions" and was predictably described as "the base and foundation of the state, on which the edifice of government is raised." Problems arose at this point, caused by the introduction of the contract metaphor. The laws, it appeared, were not laws at all. Rather, they were conventions with obligatory force "among the contracting parties." Nevertheless, they somehow had "the force of laws." The difficulty was compounded as the entry in the *Encyclopédie* attempted to reconcile contractual fundamental laws with the logic of sovereignty. The author eventually merely asserted: "Such *laws* do not render sovereignty imperfect; on the contrary, they perfect it."[56] The problem that Scheidemantel was soon to detect in Justi is apparent here. Fundamental laws as contracts presuppose the agreement of parties already possessed of rights. What the parties establish by right, they may also rightfully disestablish. If the rights of sovereignty are established by contract, then sovereignty must be imperfect.[57]

It was in this context that Rousseau insisted on the need to divorce fundamental law from the contract metaphor. Hobbes and Pufendorf had done so before, but their concern had been to deny that fundamental law imposed any legal obligation on the exercise of sovereign power. Rousseau did so in the name of popular sovereignty. At the end of Book 3 of *Contrat social* (1762), Rousseau concluded: "There is in the State no fundamental law that cannot be revoked, not excluding the social compact itself; for if all the citizens assembled of one accord to break the compact, it is impossible to doubt that it would be very legitimately broken."

[55] Daniel Oster, ed., *Montesquieu: Oeuvres complètes* (Paris, 1964), 532–36.

[56] Denis Diderot, *et al.*, *Encyclopédie*, 35 vols. (Stuttgart, facsimile edn., 1966), 9: 660. The relevant passages in Jean Jacques Burlamaqui's *Principes du droit politique* (Amsterdam, 1751) are part 1, chap. 7, sects. 36–39, 46.

[57] Justi, *Natur und Wesen*, 100.

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Rousseau's point, of course, was not to deny that there were such things as constitutional laws. Quite the contrary. Earlier in *Contrat social*, he had referred to the *loix fondamentales* as those laws that regulate the relations between sovereign and state. And in the *Fragments politiques*, he commented that the fundamental laws are laws "which magistrates themselves are obliged to obey."[58] Rousseau's point, a very significant one for the history of fundamental law, was to deny that the constitutional laws which were binding on magistrates needed to be understood as contracts. The concept of popular sovereignty admitted no such thing as laws contractually established and contractually binding on the possessors of sovereignty. Not even the social contract carried such an obligation. The people as sovereign were superior to the state. This was the conclusion that provoked extremely hostile reactions from the constituted authorities of eighteenth-century Europe.

The official reaction in Geneva is particularly relevant here. The Genevan condemnation of *Contrat social* turned, in part, on a disagreement about the nature of fundamental law. In early June of 1762, Geneva's *Petit Conseil* ordered all copies of *Contrat social* and *Emile* that were in the city to be seized. During the debates that followed, the assembly took special exception to Rousseau's discussion of periodic assemblies in the last chapter of Book 3, the chapter in which Rousseau subordinated fundamental law to the sovereign will of the people. Jean-Robert Tronchin, the *Procureur Général*, presented the case against Rousseau. He suggested that Rousseau had correctly upheld the contractual origin of political authority but had erred in refusing to accept an "engagement réciproque" between government and governed. By failing to do so, Rousseau had removed the foundation of all fundamental laws, all "lois constitutives," since all laws and governments could be changed at the people's pleasure. Rousseau had not been contractualist enough. In denying the contractualist basis of fundamental law, Rousseau had (supposedly) rendered all constitutions unstable. That Rousseau was concerned with the logic of sovereignty seems to have escaped Tronchin's attention. Rousseau's advocacy of periodic assemblies was read not as an attempt to prevent would-be tyrants from breaking fundamental laws but as a denial of fundamental law altogether. Periodic assemblies, Tronchin emphasized at one point, were "expressly forbidden" by Genevan law. It is hardly surprising that Tronchin and the *Petit Conseil* should have rejected Rousseau's reworking of fundamental law. The *Petit Conseil* itself, in 1734, had adopted a document that characterized Genevan fundamental law in precisely the contractualist way that had been common since the late sixteenth century.[59] They were clearly not concerned with the problem of divided sovereignty implicit in contractualist fundamental law, a problem that, as we have seen, troubled Rousseau, the author of the *Encyclopédie* article, Scheidemantel, Justi, and Pufendorf, among others.

[58] Rousseau, *Oeuvres complètes*, 3: 436, 393, 394, 491. The translation from *Contrat social* is G. D. H. Cole's.

[59] R. A. Leigh, ed., *Correspondence complète de Jean Jacques Rousseau*, 45 vols. (Geneva and Madison, Wisc., 1965–86), 11: 296, 298–99; the Genevan document is quoted in R. Derathé, "Les réfutations du 'Contrat social' au XVIII[e] siècle," *Annales de la société J.-J. Rousseau*, 32 (1950–52), 18–19.

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Rousseau's solution to that problem, because it undermined the *Petit Conseil*'s concept of fundamental laws, was anathema. But then, by the same token, Rousseau could plausibly maintain, as he did in the *Lettres écrits de la montagne* (1764), that the Genevans had missed his point.[60]

Rousseau's concept eventually proved to be the more persuasive one. In rejecting current notions of fundamental law and attempting not to compromise what he understood as the logic of sovereignty, Rousseau subordinated fundamental law to the sovereign will of the community. With this decision, much more directly than did Montesquieu, Rousseau demolished the elaborate intellectual constructions that identified fundamental law not only with contracts but also with the ancient foundations of the state edifice. Gabriel Bonnot de Mably, in his *Observations sur l'histoire de France* (1765), spread the message. "No-one is sufficiently ignorant," he declared, "as to confuse the first laws that a nation ever had with its fundamental laws; the fundamental law of a state is certainly not a collection of abolished, forgotten or neglected laws, but the law which regulates, prescribes and constitutes the present form of government."[61]

It was in America that this new concept had its most profound impact. How directly the lines of intellectual inheritance lead from Rousseau or Montesquieu has not been determined. But it is sufficient, in the present context, to note that the concept of fundamental law which emerges from the pages of the *Federalist Papers* (1787–88) is much closer to Rousseau's than to any of those that had appeared before.[62] The language of fundamental law had, of course, long been employed in America, and American writers had received and reworked much of the European literature with which I have been concerned here. The law-contract and foundation-edifice metaphors were as familiar in the context of American political usage as anywhere in Europe, no doubt partly because Americans continued to adopt European styles of constitutional thought, and partly because of practical circumstances. The attempt to unite thirteen colonies invited discussions in terms of bargaining, contracting, and building new foundations. Nevertheless, in Alexander Hamilton's contributions to the *Federalist Papers*, especially the famous Paper 78, an understanding of fundamental law in a recognizably modern sense of positive, constitutional law, devoid of all metaphors of pillars or pacts, is evident. He begins with popular sovereignty. It appears as "that fundamental principle of republican government which admits the right of the people to alter or abolish the established Constitution whenever they find it inconsistent with their happiness." The foundation, then, is no longer a column or contract but a principle. Hamilton insisted, in its normal operation, that "a constitution is, in fact, and must be regarded by the judges as, a fundamental law . . . They ought to regulate their decisions by the fundamental laws rather than by those which are not fundamental . . . Accordingly, whenever a particular statute contravenes the

[60] Rousseau, *Oeuvres complètes*, 3: letter 6, 811–12.

[61] Gabriel Bonnot de Mably, *Collection complète des oeuvres*, Guillaume Arnoux, ed., 15 vols. (1794–95; reprint edn., Aalen, 1977), 1: 123–25.

[62] Alexander Hamilton, James Madison, John Jay, *The Federalist Papers* (1787–88; reprint edn., New York, 1961), 167, 467–68.

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Constitution, it will be the duty of the judicial tribunals to adhere to the latter and disregard the former."[63]

WITH MONTESQUIEU, ROUSSEAU, AND THE AMERICAN REVOLUTION, recognizably modern understandings of fundamental law emerged. The subsequent history of the language of fundamentals must be a matter for another occasion. Yet, the implications of the story I have sketched thus far are quite remarkable. First, modern understandings of fundamental law emerged slowly over a considerable length of time. Contrary to the assumptions of historians of constitutional theory, the term fundamental law entered European political discourse in a relatively unconsidered way. The variety of meanings that it originally carried became less loose and more considered, in the course of inquiries that the term invited into the character of legal foundations, the relationships between laws and contracts, and between foundations and superstructures. Second, contributions to that development included those of thinkers from several European countries and, eventually, America. American writers on constitutional law were in this sense heirs to a general European, not just an English or Anglo-French, tradition. Third, the history of fundamental law in the early modern period is much more closely tied to European contractualist thought and to the imagery of political arrangements as buildings than has generally been recognized. Fourth, given the fact that we have encountered many European writers attempting to integrate concepts of ancient constitutions and customary law with contractualist ideas, conventional views of the role of custom in early modern jurisprudence would seem in need of reworking. And, finally, the history of fundamental law that I have sketched contains something of a paradox. The originality of Montesquieu, Rousseau, and Hamilton consisted, in part, in their rejection of law-contract and foundation-edifice metaphors. Yet, it was precisely these two metaphors that were decisive in accounting for the first appearance of the term, its subsequent appeal, and the manner in which the language of fundamental law was theorized for more than a century and a half.

[63] See, for example, the excellent discussion in Bernard Bailyn, ed., "General Introduction: The Transforming Radicalism of The American Revolution," *Pamphlets of the American Revolution 1750–1776*, 2 vols. (Cambridge, Mass., 1965), 2: 20–202. Hamilton, *et al.*, *The Federalist Papers*, 467–69.

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Fertility and Cultural Change among Families in Italy and America

JOHN W. BRIGGS

SCHOLARS HAVE OCCASIONALLY SOUGHT EVIDENCE of cultural persistence, or conversely, assimilation, in the fertility patterns of immigrant women.[1] When applied to Italian immigrants, this endeavor has led to incomplete and confusing findings. At the same time, it has revealed fundamental difficulties facing researchers interested in distinguishing between persistence and change. Observers of Italian immigration at the turn of the century, and later, scholars, reported that Italian women in America procreated at a rapid, and, in the eyes of many, an unseemly rate. They had large numbers of children, much as their mothers were presumed to have done in Italy. Modernizing forces in America did not appear to change the reproductive behavior of these women. Although there was broad agreement that Italian-American birth rates were high, the significance writers gave to these high rates varied. Many social reformers and critics interpreted the high fertility of immigrants as an indication of Italian moral or character deficiencies and an inability to profit from advantages that life in America offered. American social workers, many of whom held very low opinions of the capacity of Italian mothers to rear their children properly, saw high birth rates and large families as important obstacles to Italian progress in the New World. A few observers, however, offered more optimistic assessments. The author of a 1904 article in *Charities* magazine pointed out that the birth rate among Italian-Americans was much higher than in Italy and that, although the age structure of the immigrant community probably accounted for much of the transatlantic change, this high fertility was nevertheless "an indication of the economic possibilities, and value, of the group." It was also proof that, despite the difficulties of life in American cities, conditions in the New World were unquestionably an improvement over those in rural Italy. At about the same time, Kate H. Claghorn, drawing on her experience as a social reformer in New York City, expressed the hope that second-generation Italian women, benefiting from American education,

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[1] Peter Uhlenberg notes the neglect of this line of investigation in "Demographic Correlates of Group Achievement: Contrasting Patterns of Mexican-Americans and Japanese-Americans," *Demography*, 9 (Spring 1972): 119.

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would abandon their mothers' pattern of marrying early and having many children.[2]

In the 1940s, sociologist Arnold Rose addressed the possibility that birth rates might provide important evidence on the degree to which immigrants surrendered old cultural traits for those of their new homeland. He reasoned that Italians who migrated from a land of high birth rates to one of low rates should manifest a significant drop in fertility if assimilation were the only influence at work. Using data from a survey done in 1895, in which women of Italian families in Chicago reported the number of children born to them, Rose made plausible linkages between their fertility before and after migration. The data also permitted him some control over age, an important factor in fertility statistics, since fecundity of women decreases with increasing age. By subtracting children born in Italy from all children born to the reporting women and using the number of years that they were married as a crude control for age, he calculated that women in Chicago had about 25 percent more children than they would have given birth to had they remained in Italy. Rose concluded that, instead of adopting the American norm of limited fertility, immigrant families indulged in the Old World preference for large families to a greater extent than was possible in their homeland.[3]

Since the publication of Rose's work, sociologists, historians, and demographers have uncovered additional evidence and drawn divergent conclusions. Historian Virginia Yans-McLaughlin used the data on large families reported in state and federal census manuscripts to support her general argument that Italians in Buffalo, New York, learned to adapt to life in urban America without abandoning their cultural traditions. Sociologist Paul J. Campisi reported a steady progression of changes that led Italians in the United States to adopt modern urban family forms. According to his account, southern Italian peasants married in their early teens and had many children—ten was not unusual. First-generation southern Italians in America married in their late teens or early twenties and had a fair number of children—ten was unusual. The second generation married in their early or middle twenties and had few children—ten was rare.[4]

Several other studies have reported the same movement toward modern American norms. Prominent among these is the work of Italian demographer

[2] *Charities*, 12 (May 7, 1904): 445; Kate H. Claghorn, "The Italians under Economic Stress," *Charities*, 12 (May 7, 1904): 501–04. Articles in this issue encompass the full range of contemporary opinion concerning the Italian immigrants. Florence Kelley in various essays, such as the one included in this collection, typifies the social worker who found little favorable to say about the Italians; Kelley, "The Sordid Waste of Genius," *Charities*, 12 (May 7, 1904): 453–55. Also see Kate H. Claghorn, "The Foreign Immigrants in New York City," *Reports of the Industrial Commission on Immigration*, 15 (Washington, D.C., 1901), 476. The article by Phyllis H. Williams, *South Italian Folkways in Europe and America* (New Haven, Conn., 1938), 105–06, is an influential statement on Italian immigrant fertility.

[3] Arnold M. Rose, "A Research Note on the Influence of Immigration on the Birth Rate," *American Journal of Sociology*, 47 (1941–42): 614–21. For a similar suggestion that greater resources in America allowed immigrants to achieve aspirations denied them in their homeland, see Dino Cinel's speculation on the causes for greater fertility among southern Italians as compared to their northern sisters in San Francisco; Dino Cinel, *From Italy to San Francisco: The Immigrant Experience* (Stanford, Calif., 1982), 188.

[4] Virginia Yans-McLaughlin, *Family and Community: Italian Immigrants in Buffalo, 1880–1930* (Ithaca, N.Y., 1977); and Paul J. Campisi, "Ethnic Family Patterns: The Italian Family in the United States," *American Journal of Sociology*, 53 (1948): 434–47.

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Massimo Livi-Bacci. He reported that the fertility rates of Italian immigrant women had fallen below those of native-born white women by 1936, and he concluded that the immigrants rather quickly conformed to American reproductive behavior. Demographer Ira Rosenwaike found that high fertility "typical of women of peasant background" characterized first-generation Italian immigrant women in the United States. The American-born daughters of Italian-born parents showed dramatic decreases in number of births (among ever-married women who were close to the end or beyond their childbearing years). They reduced their fertility below that of most other categories of native-born women.[5]

Taken as a whole, these studies and reports exemplify several difficulties common in the assessment of the degree of cultural continuity and change among migrant peoples. Researchers must choose a baseline from which to judge change, and they must locate evidence from different times, places, and groups that is similar in level and intensity of observation. These steps are necessary if researchers are to sustain defensible comparisons. In the studies referred to above, the evidence ranges from impressionistic reports of the size of families and the age at marriage in both Italy and America to systematic surveys and aggregate census data. The units for comparison have included broad statements of Italian fertility, general American fertility, the records of other immigrant groups, and cross-generational patterns within the Italian-American population. Each choice has its advantages and limitations. The decision to compare immigrants with native-born women or later generations of Italian-Americans allows the use of general census data, which is reasonably comparable across groups and time. The procedure can test for possible movement toward more common patterns but reveals little about the experience of the first generation, beyond the expected conclusion that the immigrants did not immediately conform to a standard defined by the composite of rural and urban, native-born white women.

Those interested in the impact of migration on the culture of immigrants and the importance of that culture for the adjustment of immigrants to life in America find that comparisons with native-born persons prove very difficult to interpret. Yans-McLaughlin's comparison of Italians and Poles in Buffalo and Judith Smith's study of Jews in Providence, Rhode Island, demonstrate that other contemporary immigrant groups provide more useful perspectives on the first-generation immigrant experience than do comparisons with native groups. These studies offer the opportunity to observe different cultural traditions in the same environment.[6]

The question of change from Italian norms requires a comparison of similar forms of evidence from sources before and after migration. The findings that I present in this article summarize demographic and other types of behavior

[5] Massimo Livi-Bacci, *L'Immigrazione e l'assimilazione degli Italiani negli Stati Uniti* (Milano, 1961). Other examples include Ira Rosenwaike, "Two Generations of Italians in America: Their Fertility Experience," *International Migration Review*, 7 (Fall 1973): 271–80; Nicholas John Russo, "Three Generations of Italians in New York City: Their Religious Acculturation," *International Migration Review*, 3 (1969): 3–17; and Williams, *South Italian Folkways*, 105–06.

[6] Yans-McLaughlin, *Family*; and Judith Smith, *Family Connections: A History of Italian and Jewish Immigrant Lives in Providence, Rhode Island, 1900–1940* (Albany, N.Y., 1985).

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associated with important features of the life-cycle of families in a southern Italian village from 1861 to 1902 and of families established in Rochester, New York, by immigrants from the same village. This design ensures the comparability of data and a close linkage between Italy and America by tying specific immigrants to the cultural traditions of their Old World community.

BELMONTE, AS I WILL CALL THE VILLAGE, IS A SMALL COMMUNE in the mountainous center of southern Italy. (I adopt this name for the village to prevent identification of individuals or families in future, more detailed studies.) Located in the region of Abruzzi, it sits on the side of a forested mountain overlooking an arable plain. In the nineteenth century, it boasted a varied economy, but agriculture was the principal economic enterprise. The forests, not then fully depleted, provided work for a variety of woodworkers, particularly, turners or spindlemakers, and offered employment to a few forest guards. Shepherds and cow herders grazed their animals on the rough scrub hillsides of the deforested areas. Throughout the latter half of the nineteenth century, the population of Belmonte grew modestly, reaching 2,800 in 1901. Migration to Rochester began during the last years of the nineteenth century. During the first decade of the twentieth century, the movement continued at a steady pace. Before the migration ended, a minimum of one-quarter of the families in the village had sent children to establish new families in Rochester.

Belmonte, like most Italian communes, has a fine archive of vital statistics, thanks to the Napoleonic administrative reforms of the early nineteenth century.[7] In Rochester, I have drawn on parish records, state and federal manuscript census schedules, school records, city directories, and marriage license records to replicate the information assembled in Belmonte. I have employed techniques of family reconstruction to determine a variety of characteristics associated with marriage and family life in Belmonte and Rochester. The population for this study consists of 619 Belmonte families and 143 families established in Rochester by immigrants from Belmonte.[8] The facets of family life I will describe are ones over which the participants could be expected to have some control and which are likely to reveal cultural persistence and change associated with migration from a peasant community to an industrial city.

Family size has been at the center of the research summarized above and provides a natural core around which to cluster other features of the Italian family system. Families in Belmonte were not unusually large by the standards of European peasant societies. The mean number of children born to married couples in which the women had completed their childbearing and lived past the

[7] I reconstructed the demographic history of all families established in Belmonte after 1860 from the Stato Civile, Atti di Nascita, Atti di Morte, and Atti di Matrimonio (birth, death, and marriage records) and the Fogli di Famiglia (a manuscript record of vital statistics kept up-to-date through the early twentieth century). These records are housed in the communal archives in Belmonte.

[8] Family reconstruction involves producing genealogies, more exactly, demographic histories, for all of the relevant families. The data reported here represent populations (all members of a group rather than a sample of individuals from that group). Tests of statistical significance are unnecessary for such data.

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age of forty-five was 4.98. The inclusion of illegitimate children born to women in the same category raises the average slightly to 5.1 children. This number represents the outer limits of fertility for the village as a whole. If we employ the more common category of all completed families, the average drops to 4.74 children. Six percent of the married couples were childless. An equal proportion had ten or more children. The maximum number of children born to a woman who had completed her family was twelve. Seventy percent of the completed families had from two to seven children.

This is not the runaway fertility often portrayed in impressionistic accounts and employed as the standard to judge Italian-American reproductive rates. Although contemporary observers gave widely diverging accounts of the fertility of southern Italian women, the best evidence indicates that those in Belmonte were not extraordinary. Medical officers in Basilicata in 1878, responding to a questionnaire on the condition of the agricultural population, presented very different pictures of the fertility of women in their otherwise similar communes. From Maratea, a commune of 5,600, the mayor reported in the survey that fertility was high, with women on average bearing from eleven to fifteen children. The report from San Paolo Albanese (now Casalnuovo Licano), population about 1,300, estimated that women there typically gave birth to ten or twelve children. At Teana, a small commune of about 1,300, the medical officer calculated that peasant women ("contadini") generally had from four to six offspring. It was rare for families there to have more children, but many families were smaller. Other officials reported "modest" or "not very great" fertility. Several responses implied the presence of personal agency, noting that fertility depended on the inclination and health of the couple. A variety of statistics affirm the observations of modest fertility.[9] Figures for the Abruzzi region produced by Livi-Bacci indicate a mean number of 4.26 children per marriage in the decade 1890–99. His calculations include women who did not survive their potential childbearing years and thus produce a somewhat lower figure than those I employ.[10]

A crude measure of fertility in individual communities can be made from evidence in manuscript census records. In the commune of Craco, Matera, with a population of 1,821 in 1861, married women aged thirty-five to thirty-nine had an average of 2.4 children in their households. Mothers aged forty to forty-four had an average of 3.1 children. The comparable figures for the larger town of Rivello, population 4,412, were 3.2 and 3.4. These are underestimates of the total births. Children yet to be born, those who died young, and those who had left the

[9] "Questionario per lo studio delle condizioni igieniche e sanitarie civili ed economiche dei lavoratori della terra in Italia," Tracciato del Dottore Agostino Bertani, Commissario parlamentare per l'inchiesta agraria, 1878, Archivio di Stato, Potenza (hereafter, ASPotenza), Prefettura di Potenza, Gabinetto, cat. 24, 439–65. There are reports from twenty-three communes in this folder. The reports were summarized by the sub-prefect and sent directly to the parliamentary commission conducting the study, where they became grist for the thirteen volumes of the 'Jacini inquest,' *Atti della giunta per l'inchiesta agraria e sulle condizioni della classe agricola* (Rome, 1881–86). One must wonder how the sub-prefect and the commission chose among the greatly varied, impressionistic accounts of the "respectable intelligent persons" who were chosen to report from the villages.

[10] Massimo Livi-Bacci, *A History of Italian Fertility: During the Last Two Centuries* (Princeton, N.J., 1977), 90.

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home are not counted. (The first two sources of undercounting are probably more significant than the last.) Whatever the number of the children not counted, it cannot be large enough to produce the figure of ten or more children per woman reported in subjective accounts. Data on age at marriage and the age distribution of mothers giving birth make it improbable that the women, on average, could have had more than two or three additional children each. It is also significant that the largest number of children in a family was nine in both Craco and Rivello.[11]

What of the immigrants from Belmonte who established families in Rochester? Their fertility overall was not very different from that of their counterparts in the home village. The average for married women at the end of their childbearing years was 4.54 children. In comparison with women in Belmonte, a little more than half as many women in Rochester were either childless or mothers of ten or more children. The greatest number of births was eleven. Nearly 87 percent of all families had from two to seven children.[12] If the analysis ended here, one might conclude that while the immigrants did not produce the increase in fertility reported by Rose neither did they greatly reduce their family size below the standard of their home village. That conclusion would be premature, however. Some significant changes in the relation of social status to fertility, birth intervals, age at marriage, timing of weddings and conceptions, and rates of premarital conceptions lie hidden behind these figures.

In Belmonte, as in rural Italy generally, before the transition to modern fertility patterns in the twentieth century, large families were the prerogative of the wealthy.[13] The *benestante* (well-to-do agricultural proprietors) had an average of 6.1 children, while peasants averaged 4.6, and artisans had a mean of 4.7. Only the very small middle class (in this population, professionals, business people, and government officials) deviated from this equation of wealth and large family size, with an average of 4.5 children. In Rochester, blue-collar families averaged 4.6 children, while white-collar families had a mean of 3.9. The immigrants, in other words, moved toward a reverse pattern in which the largest families came from among the poor unskilled or semi-skilled wage-earners, a pattern that would become common in Italy a generation later.[14]

[11] Manuscript census schedules for Craco, Matera, and Rivello, Potenza; ASPotenza, Intendenza no. 1338, "Stato di populazione per l'anno 1860." The data for Craco includes all of the commune's 473 households; that for Rivello is derived from a 30 percent sample of the 1,170 households in the commune.

[12] Josef J. Barton, *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City, 1890–1950* (Cambridge, Mass., 1975), 130, presents family reconstruction data for Italian families in Cleveland, Ohio. The slightly lower figure of 4.29 children born to Barton's population of immigrant families may be due to the inclusion of families that terminated childbearing through the death of one spouse or through separation. My selection procedure is intended to discover the outer limits of fertility where couples live together for the whole period of the woman's capacity to conceive. Cinel, *From Italy*, 180–88, presents several different kinds of data on fertility with which my data is basically consistent.

[13] Livi-Bacci, *Italian Fertility*, chaps. 6 and 7.

[14] Barton, *Peasants*, 130, reports a similar pattern.

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THE TIMING OF MARRIAGES AND CONCEPTIONS in the two communities reveals further subtle but important changes in cultural patterns. A clustering of marriages in certain seasons of the year has been noted in nineteenth and early twentieth-century European peasant societies.[15] In Belmonte, weddings of couples marrying for the first time were very heavily concentrated in the summer and fall. Eighty-one percent of all of these marriages took place in the five months from June through October. By contrast, the period of similar length from December through April witnessed only 9 percent of all weddings. July was the most popular month, with 25 percent of all nuptials. August and September followed in order of popularity. The observance of a religious calendar, with its holy seasons, may have helped reduce activity around Christmas and Easter. There must, however, be other effects at work. January is the month with the fewest weddings (1.4 percent), and no month between December and April had as many as 3 percent of a year's ceremonies. The pattern of agricultural life also influenced the timing of weddings. Among agriculturalists, 85 percent of all marriages took place during the five-month period of summer and fall. For non-agriculturalists, the figure dropped to 65 percent. One way to portray the clustering is to compare it to a hypothetical situation in which the weddings were evenly distributed throughout the year. By this standard, the weddings of agriculturalists were nearly twice as concentrated as those of non-agricultural workers in the five-month period. The middle class scheduled their weddings during the "slow" periods, perhaps wishing to avoid competition for community attention from the agriculturalists.

In Rochester, immigrants changed this tendency by distributing their weddings fairly evenly over the year. March, with 2 percent of all weddings, and December, with 6 percent, were the only real exceptions. The period of June through October accounted for 45 percent of the weddings or what would have been only 8 percent above the proportion for the period, had weddings been evenly distributed over the twelve months of the year. In Belmonte, the figures for June through October were 100 percent above what an even distribution would have produced. Figure A diagrams the distribution of weddings in the two communities.

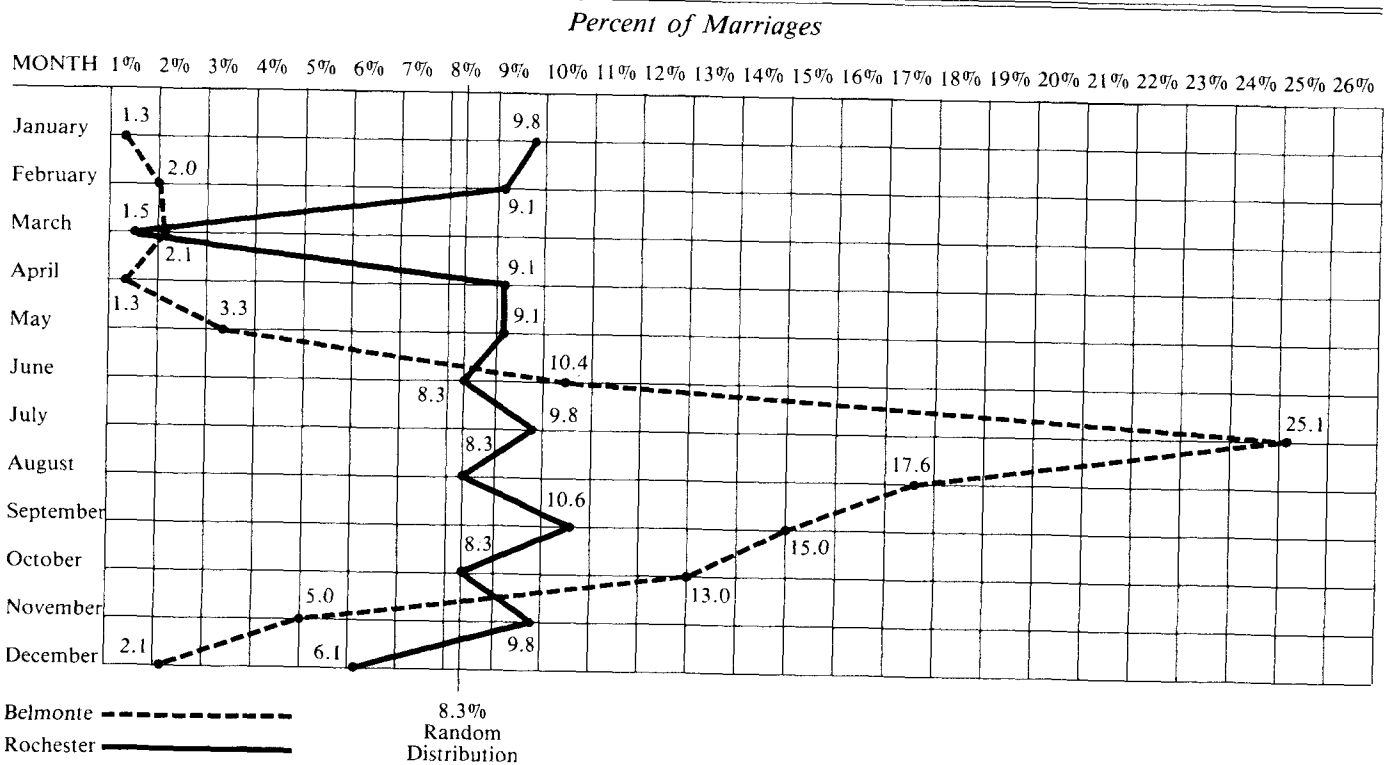
Marriages in Rochester took place almost entirely in Italian Catholic parishes, where the greatest respect for religiously based Old World mores would have prevailed. If the timing of nuptials had carried great cultural significance, the immigrants could have replicated the patterns of the Old World with little difficulty. Since they did not, we can assume that the Belmonte pattern of timing

[15] For a discussion of "marriage seasonality" among peasants in nineteenth-century Russia, see Peter Czap, "Marriage and the Peasant Joint Family in the Era of Serfdom" in David L. Ransel, ed., *The Family in Imperial Russia: New Lines of Historical Research* (Chicago, 1978), 107–08. The distribution of marriages through the year could vary considerably in communes of different size with different agricultural systems and calendars. In Sicily in 1859, 60 percent of the weddings in Contessa Entellina and 45 percent in Giuliana were held in September and October. By contrast with these small communes (ca. 3,400), residents of the larger Corleone (population 15,400) had only 28.7 percent of their weddings in these fall months. "Giunta Centrale poi Direzione Generale di Statistica, 1832–1860," nos. 51, 53, 54 (hereafter, Giunta Centrale), Archivio Di Stato, Palermo (hereafter, ASPalermo). Data presented by Rudolph M. Bell reveal considerable variation in the timing of weddings in four different Italian communes; Rudolph M. Bell, *Fate and Honor, Family and Village: Demographic and Cultural Change in Rural Italy since 1800* (Chicago, 1979), chaps. 4 and 5.

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FIGURE A

Distribution of Marriages in Belmonte and Rochester



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weddings did not result from deeply held religious or superstitious motives but from accommodations to the agricultural calendar. Industrial employment in Rochester gave no particular shape to the timing of these family events.

Associated with the tendency of agriculturalists in Belmonte to marry during the summer and fall is a tendency of conceptions to cluster in a similar manner (see Figure B). Over one-half of all conceptions occurred in the four months from June through September. This grouping represents 65 percent above what would have occurred if conceptions were evenly distributed over the year. Among non-agriculturalists, the distribution of conceptions was not nearly so skewed. Their conceptions in the period were only 25 percent greater than a random distribution would have produced. In Rochester, the distribution of conceptions over the year was about as even as such demographic events ever are (see Figure C). For the June through September period, the increase exceeds the random distribution by only 5 percent. During the six months from May through October, the imbalance in conceptions in Belmonte was 45 percent, while in Rochester it was a mere 1 percent.

The degree to which the Belmonte distribution of conceptions is satisfactorily explained by the allied predominance of weddings during approximately the same period of the year is not entirely clear. A high incidence of prenuptial pregnancy as well as a considerable variance in the period between the wedding ceremony and the birth of the first child in families in the village should suggest that the timing of conceptions is not simply an artifact of the timing of marriages. In any case, it seems likely that the Belmonte pattern of conceptions was either directly or indirectly conditioned by the social and work patterns of agricultural life. This conclusion is supported by the muted nature of the imbalance among non-agriculturalists as well as by the nearly complete abandonment of the seasonal trends among immigrants of agricultural and non-agricultural families who entered an urban labor market.[16]

Age at marriage is the second demographic characteristic by which historians have assessed change resulting from immigration. The most common assumption has been that marriage at a very young age was the norm in Italy. Over time, immigrants and their descendants supposedly delayed marriage until they approximated the standard age in America.[17] In fact, the opposite is closer to the truth. The average age of all women marrying for the first time in Belmonte was 23.6 years. For men, it was 26.3 years. The *benestante* conformed to this pattern and did not produce their larger average number of children through early marriage.

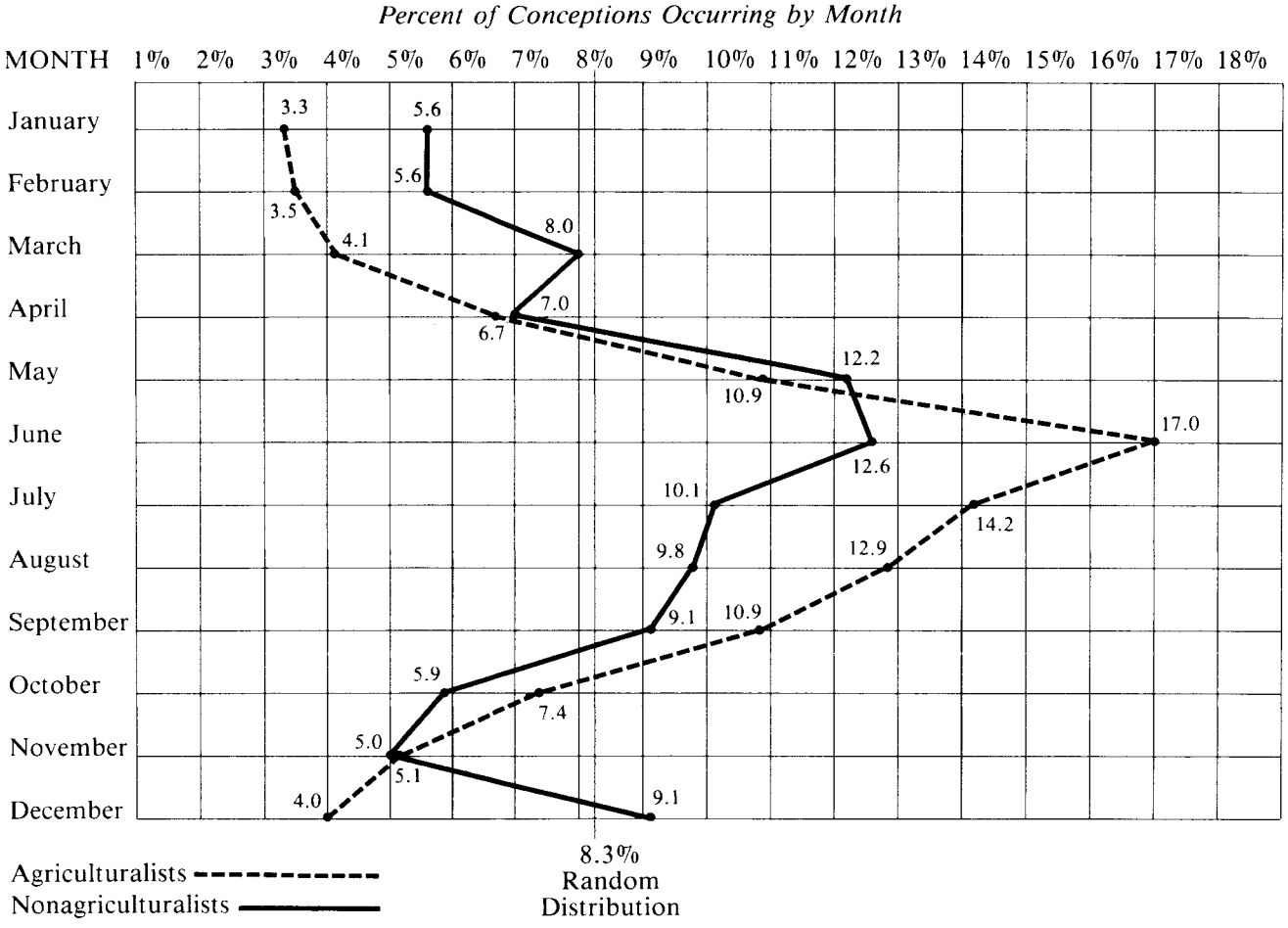
[16] Williams's claim in *South Italian Folkways*, 85, that "fetishistic beliefs" governed the choice of months for marriage and conceptions does not stand up well when considered against the experience of Belmonte emigrants in Rochester.

[17] Williams supported the assumption of youthful marriage in Italy with her assertion that "marriage took place at an early age, with the bride sometimes as young as fourteen and the groom usually only a year or two older." According to Williams, a girl who failed to marry by the time she was twenty lost her opportunity to marry well. Only less desirable mates were available for these older women; *South Italian Folkways*, 83. Williams's quote, though not specifically attributed, is from nineteenth-century Sicilian folklorist Giuseppe Pitre's *Biblioteca della Tradizioni Populari Siciliane*. Cinel, *From Italy*, 291 n. 98, quotes Pitre.

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FIGURE B

Distribution of Conceptions in Belmonte



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The only variations associated with social status were that artisan men married on the average one year earlier than the rest of the population and, among the middle class, both men and women married a year later than the overall average. This deviation probably helps to account for the slightly lower fertility of the middle class. These two variations are minor and do not significantly modify the overall picture.

Reports and data from various times and places in Italy give no consistent account of the ages of typical brides and grooms. Observers would sometimes contend that women married young, but most, including the majority of officials in Basilicata who responded to the questionnaire in 1878, believed that peasant women married at age eighteen to twenty, while men, waiting until they had completed their military service, were at least twenty-five. A more detailed report from Valsinni, Matera, contended that the age of men at marriage ranged from eighteen to forty, with an average of twenty-eight. For women, the range was fifteen to thirty-five; they most commonly married at about age twenty-five.[18] Marriage records from five communes in Sicily for the years 1840 and 1860 yield an average age at first marriage of 21.5 for females and 27.1 for males.[19] The figure for males is very close to the national average at the turn of the century. The women in these Sicilian communes married at about two years younger than the national and regional averages in the early 1900s.[20]

Immigrants to Rochester broke with this tradition. The average age of men marrying for the first time was 24.6, a decrease of nearly two years from the average for their relatives in Belmonte. The average age for women fell to 20.4 years of age, three years below the Belmonte figure.[21] Among the most important implications of this drop in age at marriage is its potential for affecting family size. Earlier marriage means that the immigrant woman in Rochester could bear a

[18] "Questionario," ASPotenza. Avvocato Domenico Giulli reported an age at marriage of from twenty-five to thirty for men and from twenty to twenty-five for women in "Memoria intorno alle condizioni dell' agricoltura, e della classe agricola del circondario di Acireale provincia di Catania/Sicilia—14 Giugno 1882," Ministero D'Agricoltura Industria e Commercio—Inchiesta Agraria no. 2, Archivio di Stato Centrale.

[19] "Giunta Centrale," ASPalermo; The Stato Civile in the communal archives of Montallegro and Realmonte, Agrigento for 1840 and 1860; Archivio di Stato, Caltanissetta (hereafter, ASCaltanissetta), Intendenza no. 442. Marriage records do not reveal the child brides said to be the norm among the peasants of southern Italy. First marriages in Belmonte involved sixteen or seventeen-year-old females 3.6 percent of the time. Four times as many women were thirty or more when they first wed. Three-quarters of the brides were in their twenties.

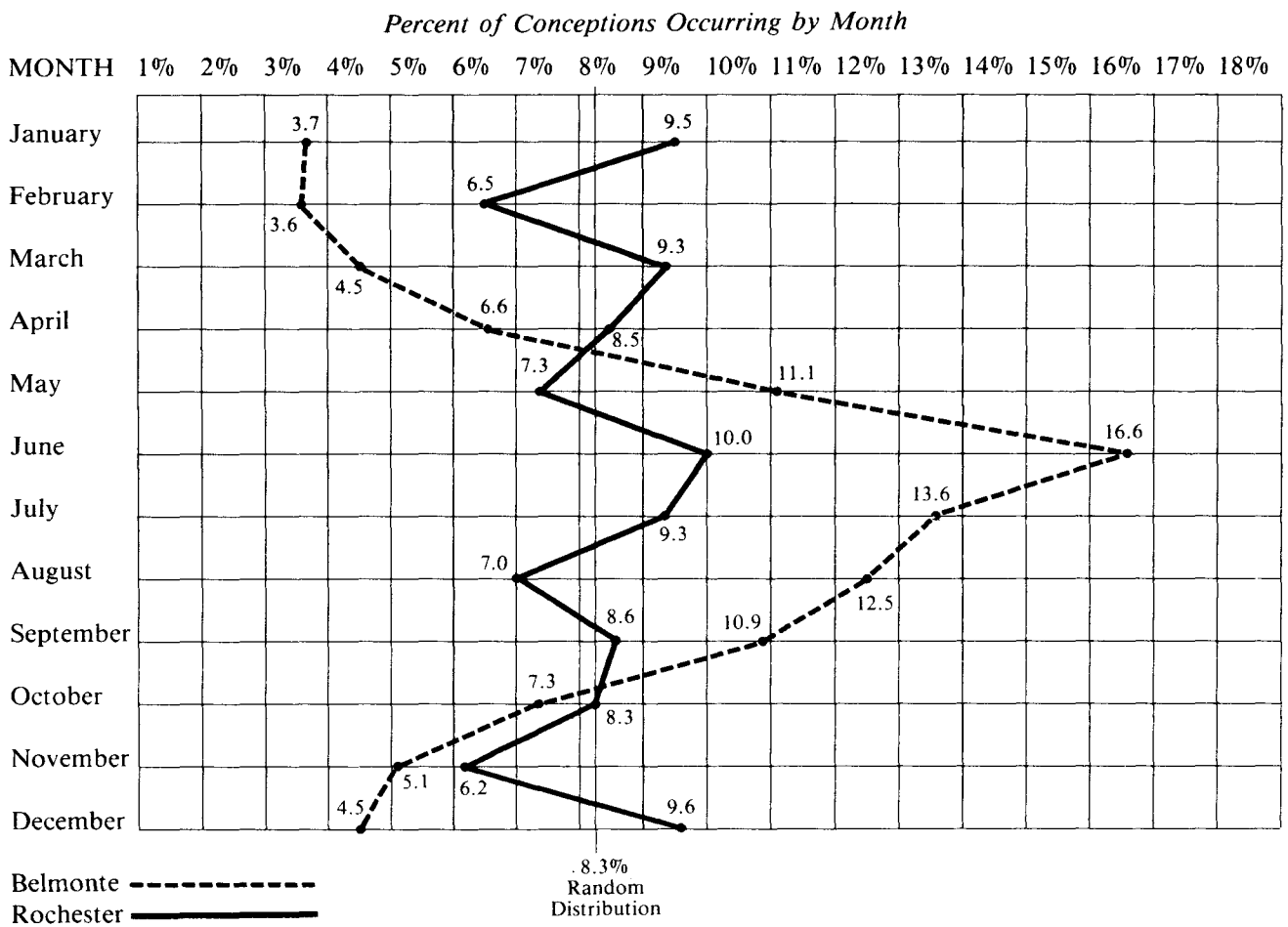
[20] The incidence of youthful brides increased in Rochester. Twenty-two percent of the brides of peasant origin were aged fifteen to seventeen. Among women of artisan and well-to-do families, the figure was 13 percent. The pattern of age differences between brides and grooms changed between Belmonte and Rochester. Seventy percent of the Belmonte brides were younger than their spouses, while 82.6 percent of those in Rochester married older men. In Rochester, the woman was significantly likely to be four or more years younger. When the man was younger, the Belmonte males were nearly twice as likely as those in Rochester to marry women four or more years older than themselves.

[21] Livi-Bacci, *Italian Fertility*, 98–107; and Bell, *Fate*, 78–105. Another calculation based on marriages from 1851 to 1857 and 1859 to 1860 in a southern Italian commune of 11,500 produced a mean age at marriage for men of 26.92 (median and mode of twenty-six years old) and a mean of 24.54 (median and mode of twenty-four years old) for women. Remarriages by men over age fifty and women over forty-five were eliminated from these statistics. William A. Douglass, "The South Italian Family: A Critique," *Journal of Family History*, 5 (Winter 1980): 350.

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FIGURE C

Distribution of Conceptions in Belmonte and Rochester



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larger family, especially since the early twenties are the years of a woman's greatest potential fertility.[22]

The fact that women in Rochester did not quite achieve the marital fertility of the women in their home village suggests that they were controlling family size to a greater extent than was the case in Belmonte. Livi-Bacci has argued that protracted breastfeeding was the principal mechanism by which Italian women moderated their fertility. It is unlikely that the immigrants could have gained any further protection from this source, since rural Italian women had already discovered the practical limits of breastfeeding as a contraceptive.[23] Family limitation in Rochester was not achieved so much by extending techniques common in Italy as by adopting new behaviors. For instance, immigrant women delayed conception longer after marrying than their counterparts in Italy did. The average interval from marriage to the birth of the first child in Belmonte was 16.1 months. In Rochester, the span was 21.2 months. Some of this difference is explained by a much greater incidence of prenuptial conception in Belmonte. Excluding pregnant brides reduces but does not eliminate the disparity. The difference between Belmonte and Rochester adjusted to include only nonpregnant brides was 2.5 rather than 5 months, or 19.9 and 22.4 months, respectively.

Because of their younger age at marriage, immigrant women began their childbearing about 2.6 years earlier than was typical in Belmonte. Once they began their families, women bore children at a rate of one every 2.3 years, until about age thirty-two. The time between the birth of the first and last child averaged 10.5 years. The interval between births in Belmonte was the same as in Rochester, while the time between first and last child was slightly longer, at 11 or more years. The most significant change leading to the greater limiting of fertility in Rochester was an early termination of childbearing. Women in Belmonte gave birth to their last children, on the average, 3.5 years later than immigrant women in Rochester did.

MODERN SCHOLARS HAVE OFTEN FOCUSED ON THE IMPORTANCE that southern Italian society places on protecting the reputation and virginity of marriageable women as well as the strain that life in urban America places on immigrant families attempting to maintain this cultural norm.[24] The experience of the groups observed here sheds some light on this issue. Overall, one-fifth of all brides marrying for the first time in Belmonte were either pregnant or had already borne a child by their husbands-to-be. Thirteen percent were pregnant at their weddings. Second marriages were rare in both communities. In Belmonte, 3 percent of the

[22] Using Italian historical data, Livi-Bacci develops a hypothetical demonstration that women marrying at age twenty-five will have about two fewer children than those marrying at age twenty; *Italian Fertility*, 13–14.

[23] Livi-Bacci, *Italian Fertility*, 16, 253.

[24] Charlotte Gower Chapman, *Milocca, A Sicilian Village* (Cambridge, 1971); Jane Schneider, "Of Vigilance and Virgins: Honor, Shame and Access to Resources in Mediterranean Societies," *Ethnology*, 10 (1971): 1–24; Peter Schneider, "Honor and Conflict in a Sicilian Town," *Anthropological Quarterly*, 42 (1969): 130–54; Jane Schneider and Peter Schneider, *Culture and Political Economy in Western Sicily* (New York, 1976); and Robert Anthony Orsi, *The Madonna of 115th Street: Faith and Community in Italian Harlem, 1880–1950* (New Haven, Conn., 1985), 137–41.

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brides and 8 percent of the grooms were remarrying. Thus, remarriage of widows in Belmonte was rare, but, when it did occur, there was a 45 percent chance that the bride had conceived a child by the man she was marrying. In Rochester, by contrast, about 5 percent of all weddings involved a second marriage for the bride or groom, and the incidence of prenuptial conception was only 6 percent among brides marrying for the first time and was unheard of among widows who were remarrying.

It is important to distinguish between premarital conception and the more socially disruptive forms of illegitimacy. Children born out of wedlock whose fathers were never officially designated or whose fathers never affirmed paternity were very rare in both communities. Only 3 percent of the women in Belmonte had children who were not eventually acknowledged by a future husband. The instances in which a woman bore an illegitimate child and did not subsequently marry were much rarer. Among immigrant women marrying in Rochester, there were no cases of illegitimate children who were not legitimized by the mother's eventual husband.[25]

Comparable statistics for other areas and times in southern Italy are practically nonexistent because data of this type can only be assembled by methods of family reconstruction. As with other questions, accounts of extramarital sexual activity were impressionistic and varied greatly. Reports from Basilicata claimed that illegitimacy was very rare or regretted that it was all too common, as in the report from Teana, a community of 1,336 that was said to have experienced a great increase in illegitimate births coinciding with the beginning of emigration to America. The author of this report was one of the most careful, insightful, and informative respondents to the questionnaire of 1878.[26]

Various counts of foundlings and illegitimate births in public records reveal Belmonte to have been typical in its share of these children. In 1875, illegitimate births in the communes of Caltanissetta province ranged from 0 to 16.8 percent of all births. The provincial average was 7.9 percent. A sample in 1880 of communes of various sizes in the province of Palermo yielded a range of 0 to 16.3 percent illegitimate births and an average of 6.9 percent. Of the births reported in the province of Calabria, 9 and 9.6 percent were illegitimate in 1871 and 1881, respectively. The rates for districts (*circondari*) ranged from 7.6 to 12.4 percent.

[25] Determining rates of illegitimacy from aggregate data is a fairly complicated process. Until 1883, Italian vital statistics included the category "foundlings," who, in most cases, were probably illegitimate but could also be the abandoned children of poor married couples. There was also the possibility of a kind of technical illegitimacy. After Italian unification, a couple who failed to obtain a civil marriage might have their children's births listed as illegitimate even if they had had a religious marriage. This occurred most often in the ex-papal states. See Livi-Bacci, *Italian Fertility*, 69–77, for an exposition of this complication in the aggregated statistics and data on illegitimacy and premarital conception. Also see Bell, *Fate*, 37–39. The figures for Belmonte do not suffer from these sources of potential confusions. There was no charitable institution in the commune where one could abandon a child, and women who wished to conceal their pregnancy had to leave the commune, most likely for a city. The offspring of irregular marriages were so noted in the birth records. Only births in which the father was listed as "unknown" are included in my count of illegitimate births.

[26] "Questionario," ASPotenza.

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Counts of pregnancy among brides in the Sicilian cities of Messina and Palermo around the turn of the century were 16.3 and 14 percent, respectively.[27]

In Rochester, premarital pregnancy occurred disproportionately among young brides. Seventy-five percent of the pregnant brides were aged twenty-three or younger. In Belmonte, less than half of the pregnant brides were in this age category. Belmonte brides in their twenties were twice as likely to be pregnant as those under twenty and those in their thirties were three times as often pregnant. Although occupation is associated with the difference in the incidence of premarital pregnancy, no occupational group was without pregnant brides. Agriculturalists, whether peasants or *benestante*, matched the overall rate of bridal pregnancy, while 26 percent of the brides of artisans and 17 percent of the middle-class brides conceived before marriage. In contrast, prenuptial pregnancy in Rochester was confined to brides of blue-collar grooms.

An additional feature of the immigrant experience in Rochester concerns the issue of premarital sexual relations. Seventy percent of the immigrant brides held jobs prior to their weddings. Yans-McLaughlin has shown that Italians disliked the idea of women, whether married or single, working outside the home and went to considerable lengths to mitigate the threat to traditional family values associated with this practice.[28] The immigrants in Rochester were successful in keeping employment before marriage from leading to personal and family dishonor. Although more than two-thirds of the women worked outside the home prior to marriage, these women accounted for only a third of the pregnant brides.

At least two factors contributed to the increased ability of families and women to observe a traditional moral code with greater rigor in the New World than in the Old. Rochester women had a decided competitive edge over their Belmonte sisters in resisting pressure to abandon their virtue. They could afford to say no without endangering their chances of marrying well. In this immigrant community in which young single males greatly outnumbered females, marriageable women and their families were in a strong position to control the terms of a courtship. Economic opportunity also altered the relationship between the sexes in Rochester. The financial independence necessary to establish nuclear families was more readily at hand in an expanding industrial city than in the largely closed economy of Belmonte.[29] Postponement of marriage in Belmonte well beyond maturity was

[27] "Movimento della popolazione," ASCaltanissetta, Prefettura, 1877; "Movimento della popolazione," ASPalermo, Prefettura di Palermo, Archivio Generale 1878–80, primo ser. no. 237, 240; ASPotenza, Intendenza no. 662; Livi-Bacci, *Italian Fertility*, 69–77; and Luigi Izzo, *La Popolazione calabrese nel secolo XIX: demografia ed e economica* (Napoli, 1965), 210–16.

[28] Williams, *South Italian Folkways*, 81, 98–99; Andrew Rolle, *The Italian Americans* (Norman, Okla., 1980), 133–34; Yans-McLaughlin, *Family*, 164–73, 201–15; Donna R. Gabaccia, *From Sicily to Elizabeth Street: Housing and Social Change among Italian Immigrants, 1880–1930* (Albany, N.Y., 1984), 47–48, 96, 98–99; and Bell, *Fate*, 123–24, which contrasts attitudes concerning women working with the reality of women's work.

[29] Responsible peasant couples had strong motivation to delay marriage until they could assemble sufficient land to support a family. With a limited amount of land available, only a portion of the couples would be successful, but all faced these pressures. Industrial employment changed this sequence of events. Once employed, a young man had little financial incentive to delay marriage. He had relatively few prospects of significantly increasing his earning power by postponing marriage for several years. For a discussion of influences governing age at marriage in an agricultural society, consequences of

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often due to limited economic prospects. Increased opportunities in Rochester made earlier marriages feasible and lessened the strain that men and women in Belmonte had experienced from the repression of sex drives under a code that dictated physical separation before marriage.

Employment opportunities for women in Rochester might have been expected to have had some effects on fertility. Employment before marriage, however, does not seem to have appreciably influenced eventual family size. Women who worked prior to marriage had only 2.7 percent fewer children than those who had no work experience. Employment of married women outside the home was decidedly less common than work before marriage was. Only 28 percent of the immigrant wives held outside jobs during their childbearing years, and some of these women worked in family enterprises. These working wives had smaller families, 3.5 rather than 4.5 children. Yet it is not clear whether these women had fewer children because they were employed or whether they were employed because they had fewer children. Whatever the cause-and-effect relationship, wives earning wages represented a significant change from the traditional role of wives in Belmonte.

TO SUMMARIZE, THE REAL QUESTION IS NOT WHETHER the immigrants maintained important cultural traditions but whether their cultural traditions stood in the way of practical accommodations to a new environment. In Rochester, unmarried immigrant women and their families took advantage of employment opportunities that allowed couples to marry earlier and relieve pressures on the traditional moral code, resulting in a greater level of conformity to that code than in the Old World. The timing of important events in family life changed dramatically, although these changes appear somewhat less striking in the light of the discovery that non-agriculturalists in Belmonte stood halfway between the immigrants and the agriculturalists. A consensus about proper family size existed on both sides of the Atlantic, but maintenance of that cultural standard required immigrant families to terminate childbearing long before their biological capacity to have children diminished and significantly earlier than was the case in their home village. "A flexible tradition," in Yans-McLaughlin's felicitous phrase, describes the cultural changes and continuities among these immigrants who, in the areas of family life explored here, neither rushed to abandon their culture nor clung to it tenaciously when doing so placed unusually heavy burdens on them.[30]

These changes and continuities become visible only after close and controlled comparisons of carefully selected evidence. The testimony of some contemporary observers pointed to patterns similar to those revealed through the systematic evaluation of demographic and census records. Other observers did not accurately

changes in economic resources following migration, and incidences of prenuptial conceptions in a northern European emigrant group, see Jon Gjerde, *From Peasants to Farmers: The Migration from Balestrand, Norway, to the Upper Middle West* (New York, 1985), 93–107, 202–05, 213–17. This book reports demographic data for Norwegian peasants in their home village and as immigrants that offer many instructive comparisons with those presented here.

[30] Virginia Yans-McLaughlin, "A Flexible Tradition: South Italian Immigrants Confront a New Work Experience," *Journal of Social History*, 7 (June 1974): 429–45.

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perceive or understand the behavior going on around them. Confusion and disagreement among immigration scholars is, in part, the consequence of their failure to agree on which observers were the more reliable reporters. Claims of cultural persistence or assimilation among immigrants are, at root, a form of comparative history. Comparative history has, despite its appeal, proven difficult to execute. Comparative historians must ensure that an assertion of a similarity or difference is authentic and not the artifact of the type of evidence employed or of differences in intensity of observations of the times, places, and cultures being compared. The approach adopted in this article—the application of rigorous, systematic methods, the standard of good comparative history—can clarify important issues in the field of immigration history.[31]

[31] C. Peter Kolchin, "Comparing American History," *Reviews in American History*, 10 (December 1982): 64–81. My claim that immigrant assimilation is best approached as comparative history might seem problematic to one who accepts the stipulation by Woodward and Fredrickson that comparative history must involve two or more countries or societies. I join Kolchin's appeal for a broader, more flexible view of what can be compared at the same time agreeing with their insistence that comparative study must be systematic and rigorous. Vann Woodward, ed., *The Comparative Approach to American History* (New York, 1968); and George M. Fredrickson, "Comparative History," in Michael Kammen, ed., *The Past Before Us: Contemporary Historical Writings in the United States* (Ithaca, N.Y., 1980), 457–73.

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The Dangers of Synthesis

ERIC H. MONKKONEN

As an occasional author of syntheses and as a grateful reader of synthesizing articles, I appreciate their challenges and virtues. They give specialists a sense of what historians in other subfields are writing. Syntheses provide a quick overview of a discipline, note important starting points and useful bibliographies. Syntheses can make comprehensible the research agendas of narrow specializations. They can set narrowly focused work in appropriate contexts and can identify materials and issues critical to our understanding of the past. Syntheses may even acquaint a larger reading public with issues in historical research, although contributions of this type are rare.

But as I work on a book about the city in United States history, a book that I had originally hoped would contain both a synthesis and an empirically based analysis, I encounter essays, seemingly everywhere, calling for synthesis, and I worry. It bothers me to read these clarion calls, particularly in media that otherwise contain virtually no high-quality coverage of serious historical scholarship. Although the people asking for synthesis often are respected historians, it strikes me as peculiar that the editors in public media can take this issue seriously when, at the same time, they consistently ignore the bulk of historical scholarship.

In the past year, I have had a deepening suspicion that this call for one kind of historical writing is, in actuality, an articulation of four mixed but understandable feelings: first, a lament that research has proliferated; second, an understandable fear of obsolescence in the face of highly technical historical research; third, a wish to get a secure theoretical position from which to view the world; and fourth, a simple hunger for more respect for what historians have accomplished. These feelings are not new, but old, recurring, and, probably, inherent to our profession. John Higham has quoted Alfred Knopf as castigating professional historians twenty-five years ago: "They have abdicated their whole position in our cul-

I wish to thank Joyce Appleby, Jon Butler, Daniel W. Howe, and Gary B. Nash for their comments and suggestions on this essay. I also wish to thank Terrence McDonald, who told me it would be professional suicide to publish an earlier version of it; he has not seen this one.

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ture."[1] One must wonder if there ever was a golden age when historians commanded the respect and readership of the many or, at least, the powerful.

Although I am in sympathy with these feelings, and on occasion share them, they are not reasonable. Research scholars will continue to write more than any one person can master. Our tradition of careful, critical reading will lead to new methods, and these innovations will produce complexity and render obsolete our established practices. Public authority and respect will come only when public discourse can accommodate subtle and complex discussions. Syntheses, however brilliant, cannot allay these latent fears and satisfy these desires. A wide readership will not necessarily bring respect; research will continue to expand; technology will continue to change; synthesizers will have to master everything.

For all the discussions about syntheses, they constitute only a small part of the contribution professional historians make to the world of learning and human understanding. The profession can survive without them, but it cannot survive without research published in monographs and articles. We sometimes feel that too much has been written on a subject, but, presumably, the authors of the works, their editors, and outside readers do not share these feelings. Trivia to one scholar may be gold to another. And when we investigate a new subject or begin a slightly different research topic, most of us discover that exactly what we want to find has not yet been done. Rather than viewing with alarm the plethora of journals, technical monographs, and empirical studies we cannot possibly read, either because of their bulk or technical apparatus, historians should appreciate the prolific community in which we participate and be glad that scholars produce more than any one person can know or know about. What a flat and boring world it would be if we could know it all.

The popular argument for synthesis and against "narrow" research can be distilled from the pages of our leading newspapers. Important historians articulate this critique, castigating others, especially historians of the United States, for their failure to create a new, comprehensive narrative of the American past. We read that a profusion of "narrowing" work has shattered overviews, jumbled the grand story's narrative, and trivialized the sweep of the past. Technical virtuosity and a cacophony of local studies have immobilized the central story just as the Lilliputians tied down Gulliver. Research of the past decade or two, we learn from the critics, has produced particularistic, fragmented results, leading to too much history and too small a reading public. The efforts expended on precision and

[1] John Higham, *History: Professional Scholarship in America* ([1965]; reprint edn., Baltimore, Md., 1983), 68. John Higham expressed optimism in 1965 that things were changing, for he blamed the unpopularity of historians on their audience, not on them. Professionals, he felt, were beginning to get the sophisticated and appreciative audiences they deserved, and he sensed that there were "advances in the present trend in listening"; p. 85.

My thought that much of the promotional literature for synthesis implicitly rejects technical skills was recently confirmed in an article by Thomas Bender, who states as fact: "History is not a technical discipline." See Bender, "Wholes and Parts: The Need for Synthesis in American History," *Journal of American History*, 73 (1986): 135. But, certainly, in the case of quantitative or linguistic skills, the best and most appropriate tools demand a great deal of their users. And, as J. Morgan Kousser stated in "The Agenda for Social Science History," *Social Science History*, 1 (1977), "artisans should fully appreciate the tools they employ"; p. 390.

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methodological sophistication have produced new knowledge but no new readers. Such research, we are told, is not only too academic and single-minded, but it lies at the heart of what is wrong with the whole history profession. If the critics are right, academic history is in serious trouble, trouble directly caused by the new interests, methods, and research that have flowered in the past two decades. Consensus historians may have ignored conflict, but my generation, I hear, has ignored its audience, the educated general reader.[2]

Given the professional interest in answering these criticisms, we can predict with some confidence that the next decade will see a plethora of narrative syntheses. The strident call that began in the early 1980s has echoed enough for the message to reach the most isolated academic retreats. Editors of history books must be eagerly looking for these new narratives. Given the strength of the calls for synthesis, a historian would have to be obtuse or wildly idiosyncratic to embark now on a carefully bounded, empirical, analytic, and, especially, quantitative, research project. "Rich," "fine-grained," "textured," "nuanced," "layered," "holistic," and "whole" are the terms to describe the wine now in demand, just as "analytic," "conceptual," "hypothetical," "methodical," and "theoretical" characterized the vintage of the past two decades.

The new banner to which our profession seems drawn, emblazoned with holism, synthesis, and book-of-the-month-club appeal, may attract large audiences, although I question whether sales (for that is the translation of "broad appeal") is what we should want. Universities and research funding agencies subsidize our work better than we care to admit; most professors have two to four months a year in which to do research. Why should these schools and foundations support authors who wish to write bestsellers? Our claim on research subsidies is legitimate because our work adds to knowledge, and this contribution is not customarily sold in the marketplace at anywhere near its production cost. Also, my bellweather sample of one—my son's cub scout den leader, and a non-academic reader of history through the History Book-of-the-Month Club—consumes a theory-defying range of books. I have seen on his shelf the names of authors as diverse as Barbara Tuchman, David Herlihy, and Umberto Eco. He also reads books on warfare by authors I have never heard of. When I asked if he were not bothered by the lack of synthesis, he did not seem to know what I was talking about and responded by recommending to me a book on medieval weapons. It was clear that he had to be categorized as a historical omnivore, consuming the fare laid down by the book club with scant attention to place, period, or method. In other words, our readers may be far more capable than we realize. As for history that depends on numerical evidence and statistical analysis, there are probably more numerically sophisticated readers outside the profession than in it.

[2] Thomas Bender, "Making History Whole Again," *New York Times Book Review* (October 6, 1985): 3, 42–43; Tia Gindick, "Frank Freidel Makes History a Good Read," *Los Angeles Times* (May 15, 1985): part 5, pp. 1, 8. In addition, for newspaper articles, see C. Vann Woodward, "Short History of American History," *New York Times Book Review* (August 8, 1982): 3, 14; and Jonathan Yardley, "The Narrowing World of the Historian," *Washington Post* (August 8, 1982): sect. C1, p. 10.

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I WISH TO ARGUE FOR WHAT IS UNFORTUNATELY BUT COMMONLY construed as fragmentation, narrowness, technicality, and low public appeal, the kind of work that is considered "building-block history." In its numerous, better forms, this is the work we are very good at; it is the essence of professional history. We should be proud of it. Focused work produces exciting breakthroughs and generates historical insights. Its test is not whether it enables others to make new syntheses but whether it contributes knowledge, new concepts, and refinements of our questions and methods. Even the dead ends are important: a project that fails to produce significant results may nevertheless reveal the weakness of certain hypotheses or the fragility of particular techniques and so indicate more effective methods and approaches.

The two subfields I know best, U.S. urban history and criminal justice history, provide instructive contrasts. In urban history, mobility studies have attracted much professional attention in the past two decades, so much attention that urban history often seemed to be only the study of occupational mobility. The "new" urban historians followed this research with a dizzying succession of article-length syntheses, historiography, and essays that have overanalyzed a thin research base without developing any major new insights or refining research techniques. Mobility research ran out of ideas without settling anything. A promising but infant field died before it reached adolescence. No visible difference to the field's vitality or purpose was achieved by what must be the greatest number of synthesizing overviews per page of research ever written. If anything, premature syntheses made unfinished business seem complete. The publicity the syntheses achieved in the larger profession did not advance the field.

Many, if not most, non-urban historians consider Stephan Thernstrom's book on mobility in Boston, *The Other Bostonians* (1973), the terminus of the "new" urban history genre. Thernstrom had energy, a sense of timing, and an eager audience, and *The Other Bostonians* attracted great attention. Its publication represented Thernstrom's heroic effort to combine particularized research with synthetic and comparative generalizations. It should have been a new benchmark, not an ending. Several excellent books and articles appeared after its publication, each improving on its predecessors. But few of the mobility studies done after 1973 earned the recognition they deserved. Despite the appearance in the 1970s of several more books that moved the study of mobility ahead methodologically and substantively, the "new" urban history was over. Scholars continued to study the less glamorous aspects of urban history, but this work was more akin to local history because of its focus on government, law, and the layout of cities. Although the writing of synthesis continued, and article after article about the "new" urban history poured out (I have counted over sixty-five), publications of empirical research apparently ceased.[3]

[3] Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973). Also see Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964). Mobility studies subsequent to Thernstrom's include, for instance, Clyde Griffen and Sally Griffen, *Natives and Newcomers: The Ordering of Opportunity*

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Self-analyses and overviews of urban history began in the 1940s. By the 1970s, the writing and publishing about urban history had created a virtual publication frenzy, climaxing in 1977 with the publication of Bruce Stave's remarkable collection of individual interviews with many urban historians. How many subfields can claim a whole book containing nothing but interviews with the leading historians of that subfield? Anyone who thinks that the "new" urban history was impersonal because its texts contained boring tables need only read Stave's interviews to discover that, when it came to cities, the historical was personal. The historians whom Stave interviewed artfully wove together biography and bibliography, concepts and careers. These interviews still convey a sense of the excitement in the field, an excitement now long gone. Writing about anonymity guaranteed high visibility for historians.

Because the "new" urban history did not reach its logical culmination substantively or methodologically, very little was settled. The methods remained crude and the results probably inconclusive. The dozens of syntheses were premature. Evidence for this assessment can be found in research by historical geographers in Great Britain. There, work of the same type pioneered in the United States has continued to attract the attention of scholars. A recent example is Richard Dennis's book on industrial cities. Advancing the methods of U.S. researchers, the historical geographers have pushed the techniques and the principal issues far ahead. Dennis's work joins with that of other British researchers in giving us new understandings of the Victorian industrial city. Even though Dennis writes as a historical geographer, the eminent historian Asa Briggs has welcomed his work into the world of British history.[4]

In the United States, two recent articles have demonstrated the inadequacy of the analytic techniques employed by mobility historians, techniques that consisted mainly of gazing at page after page of computer-generated tables. Specialists in other disciplines have developed new, if very demanding, statistical methods that could overcome the inadequacies of the earlier techniques, if only historians cared enough to make the effort to learn and use them. The effects of age and the life course, the distinct clustering of different occupational groups, the context of residential movement and its relationship to social structure, and many more problems need to be clarified. The tables that stupified the readers of the "new" urban history were unnecessarily complicated and probably misleading, but this realization should mean that other, more appropriate, techniques ought now to be tried. In other words, there is ample evidence in both substance and method that researchers abandoned the core problems of the new urban history far too early. The field fell victim to premature synthesis. Its demise demonstrates that scholars must consider carefully the relationship of synthesis to progress in

in Mid-Nineteenth-Century Poughkeepsie (Cambridge, Mass., 1978), and Peter R. Decker, *Fortunes and Families: White-Collar Mobility in Nineteenth-Century San Francisco* (Cambridge, Mass., 1978).

[4] Richard Dennis, *English Industrial Cities of the Nineteenth Century: A Social Geography* (New York, 1984).

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research and not encourage synthesis blindly. Otherwise, historians may erroneously conclude that, if the field is ready for synthesis, the research is complete.[5]

In criminal justice history, by contrast, the less frequent appearance of overviews has allowed researchers to get on with their work. The essays of synthesis in criminal justice history have served the specialists and kept them abreast of the field, but these works contain little sweep and less grandeur.[6] Fragmentation has been the order of things from the beginning in criminal justice history. The field fragmented along the disciplinary lines represented by the active researchers, who are housed in political science, sociology, criminology, American studies, history departments, and in law schools. These scattered scholarly audiences have seized gratefully on the few summarizing and synthesizing articles. Commissioned summaries in *Crime and Justice: An Annual Review of Research* have helped to put together research findings, while low-key international conferences and the annual meetings of several different disciplines inform researchers of other work. Because of the dispersal of panels of specialists among annual meetings of the American Historical Association, Organization of American Historians, Social Science History Association, American Society of Criminology, Law and Society Association, Academy of Criminal Justice, International Association for the History of Crime and Criminal Justice, and the biannual meetings of the American Studies Association—to name only those meetings at which one expects to find one or more sessions devoted to criminal justice history—scholars active in the field cannot pretend to be abreast of current research projects, much less to be expert in all aspects of their several relevant disciplines. Most significant, in stark contrast to historians of urban mobility, historians of criminal justice continue to produce dissertations, books, and articles, some strong, some not so strong. The point is, no one contemplating a research project on crime or on the criminal justice system need feel constrained by expert opinions and public statements about who really counts in the field or about the meaning of the endeavor; no one need feel smothered by syntheses of previous work.[7]

A HOLISTIC AMERICAN HISTORY OF THE SCOPE AND DRAMA FOR WHICH many apparently pine might well discourage rather than encourage inventive research projects and continued investigation of many difficult but important research problems. For instance, Thomas Bender used Charles A. Beard's *The Rise of American Civilization* to exemplify the high quality of synthesis produced in

[5] Gary W. Cox, David Galenson, and J. Morgan Kousser, "The Log-Linear Analysis of Contingency Tables: An Introduction for Historians," *Historical Methods*, 15 (1982): 152–69; and Patrick M. Horan, "Occupational Mobility and Historical Social Structure," *Social Science History*, 9 (1985): 25–48.

[6] My own overview essays include "The History of Crime and Criminal Justice after Twenty-five Years," *Criminal Justice History*, 5 (1984): 161–65; "Crime and Punishment," in Jack P. Greene, ed., *Encyclopedia of American Political History* (New York, 1984), 443–54; "The Organized Response to Crime in the Nineteenth and Early Twentieth Centuries," *Journal of Interdisciplinary History*, 14 (1983): 113–28; and "From Cop History to Social History: The Significance of the Police in American History," *Journal of Social History*, 15 (1982): 575–91.

[7] Of course, the surprising popularity and then decline of the mobility studies have more to do with external conditions than they do with the field itself. Discovering that the myth of success was not true fit well with the critical analysis of American society in the 1960s and 1970s.

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previous generations. But to use this book as a standard is to embrace the values of mass merchandising rather than to uphold the standards of our craft. There is nothing wrong with bestsellers, but Beard's work is a textbook, albeit one with interesting ideas and theses. It did not promote research. Until enough research has been done and our knowledge is adequate, trained professionals should not aim at this kind of closure. To hold up *The Rise of American Civilization* as an exemplar is to miscontrue the nature of professional history.

Scholars should remember Beard for his exciting (but definitely not bestselling) work, *An Economic Interpretation of the Constitution*, a narrow book that argued a specific hypothesis. Professionally trained historians regarded this book as Beard's most genuine historical argument. More important, *An Economic Interpretation of the Constitution* stimulated much exciting, creative, and substantial scholarship. Finally, this work, not Beard's synthesis, still deserves to be read. Most historians probably own a copy of *The Rise of American Civilization* for the same reasons I do: because it is available in every used book shop in the country, and because they have read Beard's more significant book, *An Economic Interpretation*. (Beard, of course, would have sided with the argument for synthesis, for he too considered narrow monographs arcane, backward-looking, and useless. But that is a different story.)[8]

There are other reasons to doubt some of the claims made for the virtues of synthesis. If written in the scope of Beard's *Rise of American Civilization*, a synthesis opens the way to erroneous and vacuous statements about American character and culture. On occasion, holistic synthesis is important and appropriate, but we probably do not need more than one synthesis every decade. If each fragmented subfield received a synthesis, hundreds would have to appear. Encouragement of the production of synthesis as the only genuinely sensible genre guarantees the proliferation of hundreds of useless generalizations. Imagine what would happen if a large number of historians abandoned their technically precise, "narrow," empirical monographs, and started writing syntheses. The syntheses would make little difference to our research, for specific questions and problems guide research. At best, the syntheses would serve as straw men for dissertation and article writers, or dissertations and articles would confirm the 'X' synthesis in yet another locale or for yet another period. It would be much more valuable to have researchers continue their attempts to be additive, exploratory, and inventive, to attack narrowly but precisely defined questions. The advantage of producing building-block, empirical, systematic, and methodologically explicit work is its usefulness to others. Progress in research comes slowly, but this painful process has improved the work of our profession in scope, quality, and authority over the past hundred years.

[8] Bender, "Making History Whole Again," 42. Charles A. Beard, *An Economic Interpretation of the Constitution* (New York, 1913); and *The Rise of American Civilization* (New York, 1927). Sales figures emphasize the difference in audiences between the two books: just under 8,000 for *An Economic Interpretation*, as opposed to over 130,000 for *Rise*. See Howard K. Beale, "Beard's Historical Writings," in Beale, ed., *Charles A. Beard: An Appraisal* (New York, 1954), 310–11. For Beard's antagonism toward the academic narrowness that I celebrate, see Ellen Nore, *Charles A. Beard: An Intellectual Biography* (Carbondale, Ill., 1983), 129.

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In criminal justice history, for example, one narrow, but certainly important, issue is the overall increase or decrease in crime. Subject to many careful qualifications, the index of violent crime now appears to have declined, beginning in the eighteenth century, perhaps even since the Middle Ages, until about World War II. We can propose this tentative empirical generalization only because dozens of researchers on both sides of the Atlantic have produced narrow, tedious, fragmentary, and highly specialized work. They have achieved a modicum of consensus precisely because of their independent investigations, their widely different ideological, theoretical, and methodological stances. They believe in this generalization because they know that it has been confirmed, or discovered, as the often unintentional by-product of research conducted for other reasons. They know that their increasing conceptual rigor and carefully limited approaches to the morass of uncertainties known as crime statistics have helped them say with certainty what they do not understand and cannot explain.[9]

Criminal justice history would not have progressed to the point it has if it had been designed to reach a large reading public and maintain a consensus about the course of history. More important, no one could have sensibly posed the question of long trends because of our accepted notion that there was less crime in the past. None of the traditional sources of historical research show this downward trend. Few criminologists know about it. The criminal justice system does not recognize this trend. Only investigators concentrating on narrow questions relating to the field have uncovered the evidence. Serendipity can play a role in the most disarmingly precise and narrow work far more easily than it can in global synthesis, where the author must stick to the known and believable.

Another example of the non-role of grand synthesis in criminal justice history is the non-impact of Michel Foucault's exciting, brilliant, internationally acclaimed, and widely sold, though probably not very carefully read, book on the subject of crime and punishment. Few historians in the field have taken the time to read this book with care. *Discipline and Punish* fits exactly the desiderata put forth by those who call for synthesis, yet it has not influenced research in criminal justice history. The historians are not to blame. The evidence they have amassed in the course of their research has amply demonstrated that, in his empirical statements, Foucault was often wrong. Any careful reader of Foucault realizes that Foucault's thesis has no relationship to empirical work, perhaps not even to reality.[10]

[9] Ted R. Gurr, "Historical Trends in Violent Crimes: A Critical Review of the Evidence," in Norval Morris and Michael Tonry, eds., *Crime and Justice: An Annual Review of Research*, 3 (1981): 295–353; Roger Lane, "Urban Police and Crime in Nineteenth-Century America," *Crime and Justice: An Annual Review of Research*, 2 (1981): 1–44; Nicole H. Rafter, "Prisons for Women, 1770–1980," *Crime and Justice: An Annual Review of Research*, 5 (1983): 129–82; V. A. C. Gatrell, "The Decline of Theft and Violence in Victorian and Edwardian England," in Gatrell, B. P. Lenman, and G. Parker, eds., *Crime and the Law since 1850* (London, 1980); Lawrence Stone, "Interpersonal Violence in English Society, 1300–1980," *Past and Present*, 101 (1983): 22–33; James Sharpe, "Debate: The History of Violence in England: Some Observations," *Past and Present*, 108 (1985): 206–15; and Lawrence Stone, "Rejoinder," *Past and Present*, 108 (1985): 216–24.

[10] Michel Foucault, *Discipline and Punish: The Birth of the Prison*, Alan Sheridan, trans. (New York, 1977). For an empirical analysis that portrays a reality more believable to most historians, see Patricia O'Brien, *The Promise of Punishment: Prisoners in Nineteenth-Century French Prisons* (Princeton, N.J., 1982).

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Discipline and Punish uses evidence drawn from the past to construct a holistic vision. A subtly presented work of literary art, it functions at several levels as a time bomb in the heart of modern power relations. A part of Foucault's larger exploration into the workings of power, it neither stands nor falls on its empirical representation or actual analysis of the history of the criminal justice system. Foucault's epistemological stance absolutely preempted this type of criticism. He showed that power in the capitalist age inheres in internal, individual discourses and in their verification of the bureaucracy's legitimacy. Writ large, these discourses determine our epistemology, hiding from us the truth about how power works. Only by exploding the epistemological assumptions on which it depends can power be understood. The criteria of evidence, proof, and explanation accepted by most historians are a part of the problem, according to Foucault, in that they are a part of power's duplicitous discourse. Concentration on Foucault's misunderstanding of this or that empirical detail or generalization misses the point of his work. Revolution, not research, is the appropriate response to *Discipline and Punish*. Is it any wonder, then, that the role of synthesis à la Foucault seems suspect to criminal justice historians?

I might note in passing that, as an example of synthesis, Foucault's *Discipline and Punish* invalidates one criticism of the pro-synthesis argument put forward by many, including me. The accusation that the argument for synthesis is a politically reactionary, veiled attempt to return to the history of great white men is unfair. Foucault's work illustrates that synthesis can be critical, even revolutionary. Although the cry for synthesis does seem to contain an element of political conservatism, this is because many of our syntheses in the past have recounted the doings of great white men. There is no inherent reason that a feminist, an Indian, or a Marxist synthesis could not appear. Any one of these perspectives seems more promising than a synthesis based on the public doings of powerful white men. Each would provide an Archimedian stance with which to unify the varied research of the past two decades, and each would have the energy that flows from political commitment and an eye toward political reform, the central axes of Beard and Foucault.[11]

THE PROPONENTS OF SYNTHESIS SOMETIMES IMPLY THAT WE NEED HOLISM to compensate for recent research failures. Substantive research has fragmented our sense of the whole; thus we conclude that the research has failed. But perhaps our sense of the whole is at fault. If we took our unexamined assumption about the wholeness of a national past as a hypothesis, we could send it on vacation for a few years or even into retirement. We might then consider the sense of fragmentation a result of research successes over the past two decades. What we have learned has shattered tidy images. Now that scholars of the "new" urban history have demonstrated that people moved about with unexpected rapidity, for instance, we

[11] Eric H. Monkkonen, *New York Times Book Review* (September 5, 1982): 21. Bender argues this point forcefully in "Making History Whole Again," 43.

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will find great difficulty in again thinking about a residentially stable past. We also have a sense now that the residential and occupational world of wage-earners was far more complex and uncertain than we had ever expected. These recently established generalizations cannot be ignored.

Basic historical research has succeeded in disturbing our complacency about the ease with which the human past can be discovered, described, and analyzed. We should be grateful for this revelation, not distraught. Look at how much work has been started. The large number of research projects, journals, and sub-disciplines with highly specialized techniques gives cause for excitement. The current burst in historical scholarship is unprecedented. There is far too much research and writing for one individual to comprehend, but that is no reason for despair. This outpouring is intellectually thrilling. The variety and diversity of work, the number of issues and arguments should be cause for celebration. As Darrett Rutman asked, "What is wrong with the fact that we are a disparate lot?"[12]

Historians set themselves an enormous, perhaps impossible, task. Recognizing that we cannot accomplish it quickly is no dishonor but, rather, shows an honest appreciation of historical complexity and proper humility about our own abilities. The object now is to continue the successes that flow from our diverse agendas. In the overexamined but barely begun field of the "new" urban history, success may prove difficult. Anyone who contemplates taking up where Thernstrom and a host of other able scholars abruptly left off cannot help but be aware that the work of their predecessors stands like stark ruins, abandoned by the creators and dismantled by outsiders. The intellectual landscape, left unattended by the specialists and homogenized by too many syntheses, overviews, and outside analysts, is hardly inviting.[13]

A standard criticism of analytic projects is that they reduce people to "categories, usually only one."[14] But categorization is a significant tool for achieving understanding. The standard method of teaching expository writing involves categorization. The reduction required is not trivialization, and it is not demeaning. The failure to reconstruct the categories may not be relevant if new insights, new categories, and even new questions are discovered. The most holistic synthesis must select, analyze, and relate parts to the whole: it will perforce use categories. Even the concept of a social whole is a category; as such, it excludes as well as includes. Thoreau refused to be considered the member of any organization that he had not joined; in considering him a part of the American whole, we disregard his desires. But, in some analytic contexts, we are obliged to ignore his explicit wishes and reduce him to the category of American.

[12] Darrett B. Rutman, "Comments," *AHR*, 84 (1977): 1324.

[13] Michael B. Katz, Michael J. Doucet, and Mark J. Stern, "Migration and the Social Order in Erie County, New York: 1855," *Journal of Interdisciplinary History*, 8 (1978): 669–702; Richard Jensen, "Found: Fifty Million Missing Americans," paper presented at the Social Science History Association, held in Rochester, N.Y., November 1980; and Michael B. Katz, *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975).

[14] Bender, "Making History Whole Again," 42.

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It is not surprising that a specialized literature of limited accessibility has emerged from our discipline. This specialization is the result of what Thomas Haskell called the "ruthless intensity" of a profession that has as its "native genre" the monograph. Haskell explained how the professional social sciences, especially history, emerged in the late nineteenth and early twentieth centuries as a consequence of the diminished intellectual authority of the traditional professions—law, theology, and medicine. In a quest for intellectual authority, the new social scientists, preeminently the historians, supplanted the gentlemen scholars who had been authorities on everything. Conceptualizing the world as interdependent rather than as filled with autonomous actors, the founders of the professional social science organizations instituted the behavioral categories that the departmental structures of universities now reflect. The American Historical Association and the American Economic Association formed in the 1880s, to be followed in the next thirty years by associations of the other social science disciplines. The result was multiple fields of social science with no umbrella organization or common organ of communication: history, economics, political science, sociology, and criminology, in that order, split off from the general social sciences. The barriers setting off these specialized social sciences were highly permeable.

In the century since the founding of the American Historical Association, an event that marked what Haskell termed the "emergence of professional social science," the self-scrutiny that is necessary for the discipline to have authority has continued to increase. The loss is a future without historians like Charles Beard, who, without any seeming contradiction, could make his mark in historical writing, carry through municipal reform, and become president of the American Political Science Association. But the gain is expertise. We may not enjoy a large public respectful of our authority, but historical scholarship is better than it was one hundred years ago.[15]

We have fallen from holistic grace. In the post-World War II era, we have broken too many Humpty Dumptys to do the kind of synthesizing that our prewar predecessors did. Popular syntheses must resemble textbooks, and, as such, they cannot foster research. Thomas Cochran's classic article attacking the "Presidential Synthesis" must mark the beginning of this fall. Have we forgotten its wisdom? How many active historians remember that, not very long ago, it was perfectly acceptable to periodize the American past by presidencies? We have committed the original sin of knowledge, and it cannot be undone without also undoing scholarship itself.[16] Only in the "fragmented" subfields will we see professional, research-oriented syntheses that will act as creative openings for more ideas. But these syntheses will not create a sense of the whole past. This condition reveals the achievements of historical scholarship, not its failure. Let us hope that we do not

[15] Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977): 236. See especially the section called "The Necessity for Disciplined Communities of Inquiry," 235–40, for a precise description of the role of professional discipline in modern historical research.

[16] Thomas C. Cochran, "The Presidential Synthesis in American History," *AHR*, 53 (1948): 748–59.

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sacrifice the demanding standards that the profession has achieved to a new era of popularly oriented syntheses, which, either through narrative closure or scholarly ignorance, stop or retard fledgling fields of research. And, like the scarlet letter, which was finally thought to stand for "Angel," labels such as "narrow," "technical," and "fragmented" may someday be transformed from symbols of shame to badges of honor.

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Reviews of Books

GENERAL

DAVID LOWENTHAL. *The Past is a Foreign Country*. New York: Cambridge University Press. 1985. Pp. xxvii, 489. $27.95.

David Lowenthal takes his title from the opening of a novel: "The past is a foreign country," begins L. P. Hartley's *The Go-Between*; "they do things differently there." Lowenthal goes on to explain that this realization is historically a new phenomenon: until the late eighteenth century most tended to assume that people of their own time and those who lived long ago were not essentially different. With the appreciation of real change came a new concern for preserving, interpreting, and defining one's relation to the past. The result, says Lowenthal, is that "preservation has deepened our knowledge of the past but dampened creative use of it." He says that the past is "now a foreign country with a booming tourist trade," with the result that it is becoming "swallowed up by the ever-expanding present" (p. xvii).

Such statements suggest a study that would have two well-developed dimensions. One would have to do with the perspective of the present, that is, with the ways in which the past is transformed through assimilation to present concerns and habits of perception. The other would have to do precisely with the foreignness of the past and with the difficulties a later inquirer must overcome to develop a genuine understanding of those who lived before. This would bring up the philosophical problems of cultural hermeneutics as sketched by Wilhelm Dilthey, for example, and explored by such figures as Hans-Georg Gadamer and Eric Voegelin. As it is, however, Lowenthal's book operates almost exclusively in the first of these two dimensions. He does not even mention the thinkers just referred to, although he does offer a total of three brief references to such relevant figures as Michel Foucault and Hayden White. This does not necessarily imply a deficiency, but it does indicate the absence of a dimension that would have given considerably greater depth to what, with its many excellent illustrations and its virtually indiscriminate mingling of the popular and more serious levels of culture, unfortunately borders on being a sort of high-toned coffee-table book.

I turn, however, from what this book does not do to what it does. Lowenthal discusses the ways in which modern thinkers not only have appropriated, or perhaps scavenged, the past but have also addressed questions about the possibilities of creative engagement with it. He takes up his material in seven chapters. The first has to do with expressions of historical nostalgia. The second treats ways in which the past can be perceived as a benefit or a threat to the present. The third discusses the way in which such concerns have expressed themselves in several periods as versions of a battle of the ancients versus the moderns (chap. 3). The fourth deals with the conflicting feelings, from reverence to distaste, that signs of age and decay can arouse. The fifth has to do with the ways we come to know the past. Chapter 6 discusses how we change what remains of the past even as we try to preserve it. The seventh and last chapter, "Creative Anachronism," sums up the problematic aspects of our struggle with the past and concludes that they should be willingly accepted: "Whether we restore or refrain from restoring we cannot avoid reshaping the past; no recognized vestige is devoid of present intentions" (p. 411). Recognition of this, Lowenthal says, can help demystify a history that might otherwise constrict us: "Once aware that relics, history, and memory are continually refashioned, we are less inhibited by the past, less frustrated by a fruitless quest for sacrosanct originals. . . . We can use the past fruitfully only when we realize that to inherit is also to transform" (p. 412).

Perhaps it is an expression of this very stance, however, that Lowenthal's presentation of his material is often so casual or, perhaps, "creative" as to lead to confusion and dangers of distortion. Examples abound. On page 67 he writes, "'For something genuinely new to begin,' many would echo [Mircea] Eliade, 'the vestiges and ruins of the old cycle must be completely destroyed.'" Quoted out of context

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these words are made to look like a statement of Eliade's own opinion rather than that of his sources. With a similar casualness Lowenthal writes on page 209, in a reference to Marcel Proust's *Remembrance of Things Past*, "After Proust's grandmother died, his mother appeared to take on many of her traits," as though the author and the Marcel of the novel were identical and shared the same family. At times, such sloppiness can be quite disconcerting. Reading about historical examples of people fascinated by the past, one comes across a reference to a man who "hoarded thousands of jars of his own excrement" (p. 12); one has to look in the bibliography to get a clue that this is an incident in a short story, not a reference to some work on abnormal psychology. Further on, one finds a woman who "believed herself in communion with a sixteenth-century man through a ouija board" and became so obsessed with him that she killed herself to join him (p. 30). Only a footnote to the London *Times* indicates that the incident is not fictitious but presumably factual. Some clues in the text that would help the reader keep clear what is fact and what is fiction would help correct what comes to seem a general merging and blurring of diverse material.

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JOEL C. WEINSHEIMER. *Gadamer's Hermeneutics: A Reading of* Truth and Method. New Haven: Yale University Press. 1985. Pp. xiii, 278.

The objective of Joel C. Weinsheimer's study is to make Gadamer's hermeneutic thought as presented in the philosopher's well-known *Truth and Method* (1960) accessible to a nonphilosophical readership: literary theorists, art historians, and historians proper. *Truth and Method* has a Janus face. On the one hand, the book looks backward, for it takes its departure from the split between natural and human science, which until recently was taken for granted. On the other hand, the book looks forward, for it articulates a brilliant hermeneutical approach to the materials humanists and social scientists work with.

When Gadamer published *Truth and Method*, he feared that positivism was about to dominate philosophy. As it turned out, however, the tide was already turning against positivism; in fact, his own book was an important aspect of this development. The turnabout made Gadamer's frontal attack on the predominance of the natural science model of knowledge in the social sciences obsolete. For, as Weinsheimer points out, not only in the human sciences but also in the new philosophy of science associated with Karl Popper, Thomas Kuhn, Larry Laudan, Imre Lakatos, and Karl Polyanyi, the product of the scientist's methodical investigations is no longer claimed as representing "truth." Even in mathematics, philosophers such as Bertrand Russell, Kurt Gödel, and Alfred Tarski concede that the truth of whatever is put forth as "truth" cannot be proved. Weinsheimer claims that these developments in the natural sciences strengthen Gadamer's philosophy but that Gadamer himself does not use these developments.

In the remainder of the book, Weinsheimer retells in broad outline the critiques that form the substance of *Truth and Method*: the critique of aestheticism, of historicism, and of current philosophies of language. Section 1 deals with art, which, Gadamer maintains, has to do with truth. We experience art as a source of insight. Thus, the insight that the work of art offers does not remain "outside" viewers or readers: they "apply" it to themselves. Gadamer calls this the moment of "application" of understanding. This model of understanding art Gadamer then uses for understanding history, or tradition, to which the second section is devoted. Gadamer rejects Wilhelm Dilthey's notion that the works of art, philosophy, and religion, together constituting a civilization's tradition, are expressions of life—that is, that they tell us not about life itself but merely about the subjective ways man has experienced life. Gadamer states that such works indeed do disclose truth about life and that to understand such texts implies the "applications" of their insights to oneself in order to integrate them with one's own thought. This Gadamer calls "fusion of horizons." In the formation and continuation of a tradition such "fusioning" happens all the time; inversely, all understanding of the tradition's texts is influenced and "formed" by the tradition in which the reader or the student of history happens to stand. This Gadamer calls the "effective history" of a tradition's texts. Preformed by culture and saddled by "prejudgements," human reason cannot but be historical. Such prejudgments can be evaluated positively, but only when, as Gadamer affirms, the tradition from which they stem has to do with truth. The historicity of reason, however, does not lead Gadamer to historical relativism as it did the historicism he criticizes, for Gadamer maintains that, thanks to the possibility of a fusioning of horizons, human beings can learn from each other. They are not imprisoned in their present or locked into their immediate tradition: they can expand their horizon and can, moreover, have a say concerning the direction in which they want the tradition and history of their culture to continue.

Section 3 regards Gadamer's philosophy of language, which is actually the foundation of his claim of truth for the works of art, philosophy, and religion. For historians this part of *Truth and Method* is particularly hard reading, but here, as in sections

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1 and 2, Weinsheimer performs an admirable tour de force in clearly outlining Gadamer's line of thought. The book has a select bibliography, but I miss a bibliographical essay.

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PAUL RICOEUR. *Time and Narrative*. Volume 2. Translated by KATHLEEN MCLAUGHLIN and DAVID PELLAUER. Chicago: University of Chicago Press. 1985. Pp. vii, 208. $22.50.

In *The Rule of Metaphor* (1977) Paul Ricoeur concluded that metaphor and narrative were alike and sometimes indistinguishable. Persuaded as was Dilthey that ties of filiation united all rational disciplines in a common descent from "poetic understanding," he undertook in *Time and Narrative* to explore the kinship between "two narrative modes": the first volume was devoted to historical, the second to fictional, narrative. The third volume will disclose "the threefold testimony that is provided by phenomenology, history, and fiction concerning the power of narrative, taken in its indivisible wholeness, to refigure time" (vol. 2, p. vii).

Although Ricoeur detects "poetic understanding" at work behind all narratives, he affirms "that all the arts of narrative, and foremost among them those belonging to writing, are imitations of narrative as it is already practiced in the transactions of ordinary discourse" (vol. 2, p. 156). The key to all narrative in everyday conversation, fiction, and in the philosophy of history, he contends, is the "organization" of time. Time is a "fictive experience." It first appears as the result of a "preunderstanding"—paradigmatic assumptions common to poets and their readers about human actions. Time is also imposed when a text is created (or "configured") and again when it is read (or "refigured"). In volume 1 Ricoeur distinguished these three moments in giving temporal character to human experience as three varieties of mimesis: prefigurative, configurative, and refigurative. For Ricoeur, the formation of past experience comes about through reconstruction by the imagination: history and fiction, despite differing claims to truth in their reference to empirical reality, are closely related in their modes of reference. They both derive from the same "configurating operations," or "structuring activity," of reason.

Ricoeur maintains that the task of rendering experience into narrative is linguistic. He equates understanding with rationality and, furthermore, with the functions of definition, interpretation, and explanation, all of which, he holds, are linguistic. He sets forth the maxim, "to explain more is to understand better," and he identifies his work as a venture in narrative semiotics, built "as closely as possible" on models "used in linguistics."

These principles might have led to intensive philological analyses of individual texts. Instead, persuaded that the "almost uncountable variety of narrative expressions . . . and of classes of narrative" make any inductive analysis impractical, Ricoeur applies "as nearly as possible a purely deductive procedure, on the basis of a model constructive in an axiomatic manner" (vol. 2, pp. 29–30).

In consequence of his emphasis on texts and on emplotment as "the joint work of text and reader," Ricoeur radically discounts the role of the author. He sternly ignores statements by authors concerning their intentions and any available "biographic and psychographic information" about them.

A second consequence is the subordination of texts to the "hypothetical model" that Ricoeur has constructed "in an axiomatic manner." The three varieties of mimesis appear to be Ricoeur's creations rather than formulations discovered in texts, and, indeed, the repertoire of varieties of mimesis applied through the centuries is so rich that one wonders how Ricoeur has limited his discussion to the chosen three. The "hypothetical model" largely excludes texts of fiction from volume 2, which is dominated by a review of "the structural analysis of narrative in terms of functions and actants, from Popp to Greimas" (vol. 1, p. 56). Where historical and fictional texts are represented by theories about them, it is vain to seek comments on genres of historical fiction, such as historical novels and forgeries.

Ricoeur acknowledges that his model lacks proof, and yet he speaks of "verifying" and "demonstrating" his propositions on the evidence of theories adduced in the "semio-linguistic sciences." Together with the high level of his abstractions, the absence of points of contact with whole bodies of historical, fictional, and historico-fictional writing calls for a hermeneutical Aaron to indicate how others, outside the field of semiotics, may be able to test and apply Ricoeur's propositions.

The philosophical stance that understanding is fundamentally linguistic is vulnerable to the charge of vicious circularity. Ricoeur attempts to neutralize this accusation by asserting that his argument describes "an endless spiral" of understanding, rather than a vicious circle. Yet, his contention that "the hermeneutic circle of narrative and time never stops being reborn from the circle that the stages of mimesis form" (vol. 1, p. 76) simply multiplies the range of vicious circularity. For, in his view, the reader's refiguration of the text closes the circle formed by the stages of mimesis. Ricoeur adapts Proust's statement that readers of his book were really readers of themselves. Ricoeur's assertion that

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the text is completed within the experience of the reader and denial of the qualities of the text as an autobiographical document or as an artifact of a particular *mentalité* may indeed "dechronologize" and "relogicize" narrative, but only in the sense that the limits of chronology and logic are indefinitely multiplied to correspond with the number of readers or with the shifting stages of their "life worlds."

The argument also revives a specter that metalanguage was meant to exorcise. For, if the understanding with which the reader completes the text is linguistic, it is bound no less than the text itself by the unspeakable limits and possibilities of a particular language. Ricoeur's propositions may not elucidate narrative in a language employing seven modes of the future tense. Indeed, though an admirable technical and cognitive achievement, the translation of this book discloses fundamental discrepancies between what French enabled Ricoeur to say and the equivalents possible in English; among these is the pivotal distinction of *énonciation* from *énoncé*. The danger of vicious circularity would be averted if Ricoeur could speak of "translation" from one variety of mimesis to another, rather than of "transition," but that would invite a categorical change from linguistics to aesthetics, which Ricoeur has austerely chosen to leave aside.

A premonition of death hangs over this work, for Ricoeur's meditations have led him to conclude that "perhaps, indeed, we are the witnesses—and the artisans—of a certain death, that of telling stories" and that the paradigmatic tradition has ended in radical experiments "aimed at shattering the very experience of time" (vol. 2, pp. 28, 81). Thus Ricoeur suggests "the most serious question" posed by his work might be how "a philosophical reflection on narrativity and time may aid us in thinking about eternity and death at the same time" (vol. 1, p. 87), meaning the death of narrative culture. His account of fiction ends with the cycle, advanced by Proust, that time regained through works of art is death regained.

One great merit of Ricoeur's work is to press to an extreme the argument that, as Nancy Struever put it, "words make history," and fiction as well. Yet, if history's claim to truth rests on a correspondence with empirical reality, the status of Ricoeur's own book as a historical document becomes obscure. For all the elegance and remarkable penetration of this work, it illustrates how freely the hands of the hermeneutic clock swing in their circles when ungoverned by the flywheel of evidence—that is, by what has given history its triumph over time and its role as the mother of life.

KARL F. MORRISON
University of Kansas

CHRISTOPHER LLOYD. *Explanation in Social History*. New York: Basil Blackwell. 1986. Pp. xv, 375. $45.00.

This challenging book is considerably more complex than the title might imply. Christopher Lloyd is not concerned with a summary of the development of social history over the past two decades. Although he refers to a number of social historians, particularly in Britain, he has no interest in comprehensive treatment even of major figures. Nor does Lloyd build up to theoretical statements from the issues that have arisen in recent years within history in general or within social history. Although he refers to the lack of theoretical statement and contributes some interesting thinking on problems that have attracted active social historians—such as relationships between events and structures—he does not take the concerns of the practitioner as his starting point. In addition, some issues, such as the question of narrative, are not directly addressed at all.

Lloyd is interested, rather, in contributing to the philosophy of history, or, better, to that of the social sciences. It is not particularly clear that social history is of greater moment to him than history in general, save that social historians have done some of the more provocative work of late and that their topical overlap with sociology offers some promise that history in general does not. Lloyd is preoccupied with the phenomenon of change, and this is (quite properly) his fundamental historical concern. His book has several goals. First, it argues for a study of society that is scientific—as science has come to be understood in the twentieth century and not in the outdated objective-rationalist modes of older paradigms. Second, this science of society, to be called sociology, needs to combine a number of existing approaches: it should deal with broad issues of causation and offer theories about how society operates; it should focus on structural changes, not only in the economy but potentially in politics, culture, and other areas; and it should explore what Lloyd calls action history, that is, the way larger processes work out in more specific contexts and conceivably (though again this point is not explicitly treated) in relation to major events.

These are ambitious goals. To back them up, Lloyd intertwines two approaches. First, he sums up quite varied theoretical work in the philosophy of science and in some of the major social sciences. Familiar stand-bys, such as Clifford Geertz, Michel Foucault, or Larry Laudan, are juxtaposed with less well known contributors, such as Imre Lakatos. The discussions of major historians—Emmanuel LeRoy Ladurie, Fernand Braudel, Eric Hobsbawm, Immanuel Wallerstein—fit within this framework. Lloyd does not attempt a comprehensive summary

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of twentieth-century theory, but he competently handles a number of major schools and developments. Second, Lloyd gradually elaborates his own argument, partly through critical rendering of existing approaches. He is concerned, for example, to define the relationship between social structures and the intents and acts of people in determining social change, and he urges essentially a compromise position in which structures and "agential" humans interact. Unlike some partisans of a structural approach, Lloyd is at pains to bring in some grasp of social psychology and ritual, and not simply through Foucault's stiff archaeology of discourses (which Lloyd does not in any event greatly fancy). Lloyd warns convincingly about the limits to purely descriptive, qualitative social history that eschews concern with theory and so fails ultimately to explain. In sum, in developing a resounding call for new integrations in the study of society, Lloyd presents a number of key issues with sensitivity. And the encouragement to more informed examination of theoretical problems is certainly welcome.

Yet, I confess to a certain disappointment with the end result, despite considerable sympathy with the author's basic aims and individual passages in his book. A final substantive chapter proves somewhat conventional in reminding of the important theoretical work of sociologists such as Antony Giddens and Philip Abrams who set the stage for an integration of history and sociology that they were unable to accomplish and in praising Hobsbawm and LeRoy Ladurie as exemplars of "scientific social history," a choice that is hard to fault. But the lack of awareness of issues generated within social history, some of which overlap his own concerns, and the lack of application of his pleas and definitions to any concrete social-historical cases lend a certain vagueness to Lloyd's overall approach. At the least, further work is needed to apply this framework to the actual practice of historical research or to the generation of the theory that the book calls for.

PETER N. STEARNS
Carnegie Mellon University

EMMANUEL TODD. *The Explanation of Ideology: Family Structures and Social Systems*. Translated by DAVID GARRIOCH. (Family, Sexuality, and Social Relations in Past Times.) New York: Basil Blackwell. 1985. Pp. 230. $24.95.

A fascinating, albeit problematic, idea drives this book: ideology corresponds to basic sets of attitudes that determine what kinds of doctrines a people find acceptable—and these attitudes originate in the form and workings of the family. The family consequently takes center stage in explanations of ideology. This argument grows out of Emmanuel Todd's experience with the Cambridge Group for the History of Population and Social Structure and his work in French electoral sociology.

The author develops seven categories of families to which all world families are assigned, categories based on inheritence rules, cohabitation patterns, and the extent of the incest taboo. The bulk of the study links those family types to the political and social systems of Europe, Asia, and the Western Hemisphere. The exogamous community family of Russia, for example, which emphasizes equality among brothers and parental cohabitation with married sons, creates a receptivity to communism that is lacking in Poland where the egalitarian nuclear family reigns.

Todd's explicitly anthropological argument will be unattractive to many historians because it is ahistorical and antimaterialist. In discounting both material and intellectual contributions to historical and political processes, the author denies their credibility. "The political history of France," or anywhere else for that matter, "is a pseudohistory" (p. 121). Predictably, the argument falters most seriously in explanations of historical phenomena, such as the American Civil War and the Khmer Rouge movement. Problematic sources exacerbate the implausible aspects of the study; for example, the section on Cuban families relies on suicide rates and census data rather than on anthropological studies. Todd does not view liberty or equality as rational or universal objectives—an equanimous but controversial scholarly stance.

Yet the anthropological view is not a neutral one, for some family forms and their concomitant ideologies are judged severely. Todd sees the authoritarian family (multiple generation households with primogeniture inheritence) as a "neurotic machine" producing "psychologically pathogenic" social systems in times of stress, such as in Nazi Germany (p. 65). Community families, such as those in Russia, produce anxiety that is revealed in high suicide rates. By contrast, the nuclear family can do no wrong: incapable of supporting totalitarian regimes, it rather releases that "wonderful elementary social particle," the individual (p. 100).

The attractiveness of Todd's study lies in its global scope. Despite an attachment to certain family forms, the argument itself is a weapon against ethnocentrism and such narrow notions as the existence of an inevitable link between secularization and modernization. Moreover, Todd's far-ranging and thought-provoking argument is delivered with lucidity. Perhaps more importantly, the book will produce discourse as well as discomfort; it will generate debate on the meaning of family form,

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raise the visibility of the family in society, and encourage comparative studies.

LESLIE PAGE MOCH
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JANE RENDALL. *The Origins of Modern Feminism: Women in Britain, France, and the United States, 1780–1860*. New York: Schocken. 1984. Pp. vi, 382. $21.50.

This is an ambitious project. Examining the social context in which feminist practice became possible, Jane Rendall looks at the origins of association among women in a comparative way. The path she takes is a familiar one. Rooting the modern debate on woman's function and role in the problematic legacy of the Enlightenment, she demonstrates that women inverted the language of domesticity to claim for themselves positive educational, religious, and inspirational functions. As Harriet Martineau said, "Women pursued religion as an occupation" and thereby spurred the development of associations among them. Also integral to women's developing sense of themselves and their mission was a greater understanding of the disparity between men's and women's education and the heightened importance attached to the mother's task as educator and socializer. The demand for work and the circumstances of women's work were continuing factors in the generation of feminist thinking. Involvement in moral reform movements—philanthropy, domestic visiting, abolition, temperance, health reform, and social purity—was highly significant for women's emergence into public roles in all three societies, but in different degrees. The impact of women on the literary scene in the first half of the nineteenth century, their desire for autonomy and independence outside the married state, and the challenges to monogamy and the cult of domesticity launched by utopian socialists were all additional elements, according to Rendall, in the expansion and redefinition of women's sphere of action. Although none of this is new or original, her comprehensive enumeration of the multiple sources of feminist consciousness and her sensitivity to its differing nature and scope in all three societies are impressive.

Most noteworthy about Rendall's analysis is her ability to find meaning in heretofore-neglected aspects of women's experience. She highlights, for example, the collective side of women's domestic activities. Gathering wood, baking in the communal oven, fetching water, and meeting at a quilting bee were all important social occasions and a means of asserting female solidarity. In addition to balancing conflicting arguments and outlining varying interpretations, Rendall targets work that remains to be done: uncovering the differing marriage practices of communities rather than accepting the universal validity of bare legal frameworks; detailing the implications of the spread of piped water in changing the locus of household activities and encouraging women to stay at home; evaluating the particular experience of girls in the history of mass elementary education; tracing what became of the radicalism of Chartist women after 1850. Rendall acutely explores the subtle differences in the three national contexts and shows that the character of the women's movement owed much to the pattern of class and political conflict in each nation. Whereas American feminism became part of the general reformist ethos and British feminism was subject to the politics of class, French feminism faced particular difficulties after 1848. Rendall succeeds admirably in isolating the barriers French feminists faced. Although abolition was a powerful trigger to women's public consciousness in both Britain and the United States, the silencing of protest activity in France after 1848 and the absence of a strong evangelical network underlying the campaign served to slow the growth of antislavery sentiment. Factors that had spawned feminist activism elsewhere were also affected by the particularities of the French context. Improvement in female education was caught up in the debate between clerics and anticlerics; state regulation and toleration of prostitution inhibited the growth of a social purity crusade of the Anglo-American type.

The historical analysis of the roots of feminism is a vast topic. Although Rendall has relied primarily on secondary literature rather than on original research, she has created a useful synthesis, mined from a wealth of sources and rooted in a keen understanding of the major works in women's history to have appeared in the last two decades.

MARIE MARMO MULLANEY
Caldwell College

MANUEL CASTELLS. *The City and the Grassroots: A Cross-Cultural Theory of Urban Social Movements*. (California Series in Urban Development.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xxi, 450. Cloth $29.95, paper $14.95.

This book by Manuel Castells appears to have two authors, whom I shall call Castells (I) and Castells (II). Castells (I) is an austere, abstract, often incomprehensible, neo-Marxist theorist intent on proving that all grass-roots urban protest movements are somehow part of a single worldwide class struggle. Castells (II), however, is a wonderfully eclectic social investigator whose empirical findings are constantly breaking out of his self-imposed theoretical rigidities. Whereas Castells (I) in the theoretical chapters

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numbs the reader with such pronouncements as "experience is the action of human subjects on themselves within the multidimensionality of their biological and cultural entity" (p. 305), Castells (II) in the case studies is constantly alert to all the human complexities of grass-roots movements, aware of the cultural and gender-based conflicts that often supplant the economic struggle, and able to deal brilliantly with such diverse movements as the gay community in San Francisco and squatters in Latin America. The result is an odd mixture of turgid social theory and populist enthusiasm: Louis Althusser meets Frank Capra.

Fortunately, Castells (II) is the main author of this work, and his detailed case studies will surely be of interest to urban historians. He begins with three brief historical examples of grass-roots movements: the *communidades* of Castilla (1520–22), the Commune of Paris (1871), and the *movimiento inquilinario* in Veracruz (1922). After a discussion of black revolts in American cities of the 1960s, he turns to his major studies: the tenants' movements in the high-rise *grands ensembles* outside Paris in the 1960s and 1970s; the gay and Hispanic communities in San Francisco; the Latin American squatters; and the Madrid "citizen movement" at the end of the Franco regime.

The Madrid study is perhaps the most successful, because the citizen movement demonstrated all the characteristics that Castells believes define the grass-roots urban movement: demands for better urban services, which he calls "collective consumption trade unionism"; a search for local cultural identity; and action for local political autonomy.

Not surprisingly, Castells the theorist can do little to unify the variety of material that Castells the investigator has uncovered. What holds the book together is the author's belief in the failure of large bureaucratic organizations to shape the city for human ends and in the consequent necessity for grass-roots action if the good city is ever to be built. He is much closer to another urban populist, Jane Jacobs (who marches under the banner of Adam Smith), than either would care to admit.

Urban historians do not need Castells to direct them toward the study of urban protest movements; still less do they need his theoretical apparatus. Nevertheless, the very range of Castells's case studies makes his point more effectively than do his theories. No one reading this book can doubt that grass-roots urban protest movements are indeed a worldwide phenomenon whose significance can be understood only in the widest cross-cultural perspective.

ROBERT FISHMAN
Rutgers University, Camden

ANDREW LEES. *Cities Perceived: Urban Society in European and American Thought, 1820–1940.* (Columbia History of Urban Life.) New York: Columbia University Press. 1985. Pp. xi, 312. $30.00.

As indicated by its title, this book takes a panoramic view of its subject. It is divided into three main sections: "Urban Themes in England and Elsewhere, 1820–1880," "Perspectives on Urban Ills, 1880–1918," and "Positive Views of City Life, 1880–1918." Andrew Lees has worked through an enormous amount of material covering Britain, France, Germany, and the United States, including medical and statistical records, tracts, popular literature, newspapers, belles-lettres, and the writings of urban sociologists and architects. Lees reaches well beyond the more or less canonical sources for evidence of perceived cultural attitudes toward the city in the last century and a half. As he says in his preface, "I have sought to encompass a very large number of writers and as much attitudinal diversity as I could find" (p. ix). The author states in the first of two bibliographical essays that his initial objective "was to locate and at least to skim all the general books and pamphlets on cities written in Britain, France, Germany, and the United States and published in the nineteenth century or the first four decades of the twentieth. Omitting most works that focused narrowly on such topics as city government and city planning, I sought out those writings whose authors attempted to take stock at some length of the reasons for urban growth and the quality of urban life" (p. 346).

The author says that his subject is "consciousness rather than society" (p. x), and, although this poses certain problems in the book—marshaling so much empirical data tends to crowd out the larger cultural perspective—his viewpoint as a cultural historian is valuable. In presenting his many sources, his ear is always cocked for the political, emotional, or class biases behind them, and inclusion of these overtones gives the reader a kind of three-dimensional view of the subject. Considering the number of witnesses the author summons, this book could have become an undifferentiated monster similar to Pierre Citron's *La Poésie de Paris de Rousseau à Baudelaire* (1961), but this does not happen. The most impressive characteristic of this book is the judiciousness and sense of proportion with which Lees presents his material.

The intellectual argument is somewhat scanted. The section on intellectual inheritance is rather sketchy; the use of literature as social documentation needs, on the whole, a more sophisticated approach than is found here. In particular, the author does not account for Balzac's establishment of the parameters of sociological urban discourse. (Balzac has been accused of having invented the

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nineteenth century.) The conclusion is indefinite as a comprehensive organizing argument, but Lees's evidence points toward ambivalence, and he is careful not to force his argument.

Of great value is the comparison of shifting values toward the city among English, French, German, and American societies, which makes clear the dependence of such attitudes, in modern times, on national schemas (I use the term in E. H. Gombrich's sense), which differ from country to country and can change over time in the same country. Lees's book warns us against easy overgeneralization about the city.

The book also shows that if one takes evidence from a broad social spectrum rather than only from canonical sources, especially in literature, negative biases toward the city are at least balanced by positive attitudes. Apparently a strong negative bias has been passed down from religious and classical sources by our high culture, whereas people less indoctrinated by this culture are, on the whole, more positively oriented toward the city. The conflicts arising from this ambivalence are of central importance in assessing the city as a cultural artifact, and Lees's documentary evidence, together with his judicious presentation, furnish much information and food for thought in a form useful to scholars in the field.

BURTON PIKE
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CLAUDE WELCH. *Protestant Thought in the Nineteenth Century*. Volume 1, *1799–1870*. New Haven: Yale University Press. 1972. Pp. x, 325. $25.00).

CLAUDE WELCH. *Protestant Thought in the Nineteenth Century*. Volume 2, *1870–1914*. New Haven: Yale University Press. 1985. Pp. xii, 315. $25.00.

Volume 1 of Claude Welch's projected two-volume history of nineteenth-century Protestant theology appeared in 1972. In that widely reviewed and applauded volume he outlined a clear agenda. Key to the era was the problematic of theology itself, more specifically the questions of the possibility of Christology and the relationship of Christianity to culture. Although always interwoven, these concerns came to the fore by turns in successive periods: 1799 to 1835, 1835 to 1870, and 1870 to 1914. Volume 1 covered the first two periods. With the long-awaited second volume now in hand (and a concurrent reissue of the first volume), it is possible to enjoy and appraise in its fullness a rare magnum opus in historical theology prepared by an American scholar.

As reviewers work over these volumes, an obvious roster of complaints will emerge. It may be doubted that Welch always delivers on the large promises of his "general history." For the most part he hews to familiar (most often German) base lines from Friedrich Schleiermacher to Ernst Troeltsch; coverage seldom ventures beyond Germany, England, and the United States. He presents an exegesis of problems and texts without much reference to newer methodological fashions and in a few places descends into a mere serial canvass of thinkers. He does little to connect his theologians with the large middle range of intellectual history—the journals, quarterly reviews, tracts, treatises, and sermons, which often were as important as the few exercises in high theology in inspiring, flavoring, and expressing agendas of thought. The author concedes little to recent demands that intellectual history be grounded in social context. Deciding to avoid orthodox and conservative patterns in favor of more "exciting discussion" (vol. 2, p. xi), he makes little, for instance, of the "evidences of Christianity" or the natural theology central to actual theological curricula, at least in the dominant English-speaking traditions, through the first two-thirds of the century. He also does not treat figures such as John Nelson Darby, whose revision of Protestant millennialism inspired a virtual paradigm shift and affected the thinking of a far larger sector of international Protestant opinion in the later nineteenth century than did the great majority of thinkers treated. The final volume ends abruptly without a summary and a final stocktaking.

Yet such difficulties, many the result of necessary limitations in topic and space, do not obscure the solid achievement of these volumes. At the core of the work stands a historiographical departure of great significance. If it seems less significant today than in 1972, the salutary effect of volume 1 is part of the reason. For there Welch challenged two entrenched and often overlapping conventions: first, the attempt to tell the story of Protestant theology primarily in a context of national history; second, the virtual identification in earlier histories of nineteenth-century "Protestant theology" with its German development. Certainly there has been a national element in modern theologies, and Germany has exercised the greatest influence. Much substantial history has been written in this vein, but at the cost of coverage, balance, and understanding. German-centered history has ignored or scanted other areas and thinkers. It has valued religious thought elsewhere largely as a register of German influences and has slighted the international context of influences. The historiography of American theology offers a clear register of these tendencies. There is no adequate history of American theology in any century or major period, let alone a comprehensive survey (although one is now in preparation). There is no general agreement that a true grasp of

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the British and European antecedents and connections is prerequisite to work in the area, and few of the existing studies reflect sophisticated expertise in the discipline of historical theology. Observed against this background, with many a counterpart outside the United States, the exemplary value of Welch's project becomes apparent. For he not only sets issues and patterns within an international conspectus but demonstrates on every page a technical mastery of theological languages and concerns. His completed study now takes its useful place as the one recent, unified, and international history of the theology of any full century of the Protestant experience.

THEODORE DWIGHT BOZEMAN
University of Iowa

CARL J. PFEIFFER. *The Art and Practice of Western Medicine in the Early Nineteenth Century*. Jefferson, N.C.: McFarland. 1985. Pp. xiv, 238. $29.95.

Carl J. Pfeiffer has gathered numerous quotations, many of them of some length, from journals and texts published in English between 1800 and 1825 to bolster his thesis that this period of Western medicine provided a foundation for scientific exploration, debate, and experimentation throughout the century. During the first quarter of the nineteenth century, medical journals began to be published in the United States. From the pages of these prolix, stilted, sometimes-pompous medical journals and texts, Pfeiffer selects liberally to construct his unwieldy essay. Among the topics of the chapters are Galvinism, the medicinal leech as a bloodletting agent, concepts and misconceptions in cardiovascular physiology, smallpox vaccination, obstetrical practice, diseases of the digestive tract, influences of weather on disease, bathing in cold water and blistering, and, finally, quicksilver and other medical materials. The author expresses relief at not contracting an allergy while reading these dusty and little-consulted medical journals, but his historical assessments, which are based on disjointed and largely undigested texts, will make many readers uncomfortable. Of course, some readers may feel relieved at not having to go to the original sources.

Perhaps indicative of the usefulness of this book on the topics listed is that, in searching for specific information relating to bloodletting in ophthalmology, I found several useful pages. Aside from my own good fortune, this discovery leads me to believe that Pfeiffer developed a sense of what was most important in the journal literature and text books, even if he did not feel compelled to question the arguments with more novelty and introspection. Without dwelling on the reasons or discussing the ramifications in a particular debate, the author recognizes that many scientific and medical crises are "modulated by non scientific and non humanitarian factors of political or economic nature" (p. xiii). If these factors had been pursued in greater depth, Pfeiffer's volume would have provided a more interesting and significant historical picture of this formative period in Western and American medicine.

Pfeiffer's choice to conclude his book with an account of George Washington's last illness and treatment, published in *The Times* of December 1799, seems odd, although it would appear to have been a good opening statement on American medicine in this period. He concludes that, except for the extent of the bloodletting prescribed for Washington, the other treatments (blistering and purging), as well as the probable outcome for the patient, would have been the same in 1825. When confronted with the conclusion that many physiological concepts of the early nineteenth century were compounded of elements of truth and some misconceptions, it is not surprising that the diagnosis and treatment of disease was both effective and inadequate. Pfeiffer provides examples of the effective and misunderstood practices of medicine on all the topics he has chosen to explore. The book reflects a diligence and love of reading of original medical sources that provides a feeling for the subject, but it does not give the reader an analysis of (or, for some readers, enough justification for publishing this volume on) early nineteenth-century medicine.

AUDREY B. DAVIS
Smithsonian Institution

HUGH G. J. AITKEN. *Syntony and Spark: The Origins of Radio*. Reprint. Princeton: Princeton University Press. 1985. Pp. xvi, 347. $13.95.

Ten years ago this book won the History of Technology Society's Dexter Prize for the best book published in the history of technology. That it has remained in print and is now available in an unrevised paperback edition clearly indicates the scholarship of Hugh G. J. Aitken's original publication.

The book examines the development of radio or wireless transmission from 1887–88 to 1914. The dates correspond to the years in which Heinrich Hertz experimentally confirmed James Clerk Maxwell's electromagnetic field theory and the invention of continuous wave (triode) vacuum tubes by Lee de Forest and Edwin H. Armstrong made spark discharges obsolete for radio wave generation. The origin of radio is a case study that enables us to understand the changing creative process and to focus our attention on the ways in which the transfer of knowledge occurs. Vital to these transfers, Aitken

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says, are the individuals and institutions that perform the "translator function:" the decoding of information generated in one system and translation of it into information useful in another.

Aitken has divided his book into two parts: an analytic section consisting of epilogue and prologue in which he discusses the interaction among science, technology, and the economy, and a narrative section in which he describes the beginning of syntony (or tuning) in radio and the roles of Hertz, Oliver Lodge, and Guglielmo Marconi. They were the translators who took the idea of wireless transmission from the world of pure science to that of commerce. Hertz was uninterested in the commercial development of wireless and had demonstrated the existence of electric waves in air only because the Berlin Academy required their demonstration for its prize in 1879. He was not an innovator of theories but had a marked ability to deal with the conceptual problems others raised. Unlike Hertz, Lodge had a lifelong interest in the practical applications of physics, and he made the transfer from pure science to usable technology in 1894 when he demonstrated a system of radio telegraphy that exchanged Morse code signals. But Lodge was slow to see the implications of his work and to protect his patent of 1897 on syntony. The delay resulted partly from Lodge's interest in the ether drift problem and, as Aitken notes, from Lodge's dominant orientation to the values of science, rather than to those of the marketplace.

This set the stage for Marconi to complete the transformation. Marconi had little scientific training, but, through his friend Augusto Righi in Bologna, he had access to scientific equipment, knowledge, and literature. He also had no business experience. But, on arriving in Britain in 1896, Marconi had available business advice, capital, and contacts through his mother's family. Marconi may have contributed little that was original to the new technology, but he had the stubbornness required to succeed and an intuitive sense that enabled him to see that wireless fit into the existing economic system. He took wireless from the laboratory and translated it into an organization that in 1919 became the Radio Corporation of America. Hertz was the first to demonstrate the reality of electromagnetic waves, Lodge provided the syntony, and Marconi the spark to wireless transmission.

Aitken's interesting narrative and penetrating analysis have made this book a major contribution to the history of science and technology. His analytic section, in which he distinguishes at length between the domains of science and technology, is more controversial and is sure to spark debate between specialists in the history of those two disciplines.

ANTHONY N. STRANGES
Texas A&M University

HAROLD G. DICK. *The Golden Age of the Great Passenger Airships, Graf Zeppelin and Hindenburg*. Assisted by DOUGLAS H. ROBINSON. Washington, D.C.: Smithsonian Institution Press. 1985. Pp. 226. $24.95.

This is primarily a book for airship buffs, and a first-rate example of its kind. Harold G. Dick analyzes the design, construction, and operation of Germany's two major zeppelins in penetrating detail. Americans generally recall only the end of the airship era in the *Hindenburg* disaster of 1937. But here they will find the best English account of the forgotten zeppelin service to South America, which began in the autumn of 1931 and continued regularly over the next six years until May 1937. This venture, and its North American counterpart of 1936–37, were the precursors of modern transoceanic commercial aviation, done in a comfort and style never equaled since.

Dick provides a characteristic account of life in Nazi Friedrichshafen as seen by an American engineer unusually prvileged to work in the *Luftschiffbau-Zeppelin* as the resident representative of the Goodyear Zeppelin Corporation. He participated in more than twenty transatlantic flights of the two zeppelins and came to know Hugo Eckener well. This famous airship flyer had devoted half his life to the zeppelin cause, often under adverse financial circumstances in the Weimar Republic. Dick shows how Eckener experienced the full Faustian dilemma when the Nazis finally gave him adequate financial support for the technology but then dictated the political dimensions of his ultimate success.

The author has written an engaging personal memoir of the climactic zeppelin years. His is probably the best account we shall ever have of the relations between the American and German airship builders. The book is further strengthened by the wider expertise of Douglas H. Robinson, dean of professional American airship historians. This fortuitous collaboration is borne out in several appendixes, including a useful glossary of technical airship terms and a translation of a zeppelin crew manual. Many photographs and excellent technical drawings complete this attractive study.

HENRY CORD MEYER
University of California, Irvine

ANCIENT

K. A. KITCHEN. *Pharaoh Triumphant: The Life and Times of Ramesses II, King of Egypt*. Mississauga, Ont.: Benben. 1982. Pp. viii, 272.

In an age of ever-increasing specialization in the academic world, we sometimes long for the days

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when scholars took the time to write books for the general reader. This is all the more reason to welcome K. A. Kitchen's new book on one of the great figures of the ancient world, Pharaoh Ramses II of the Nineteenth Dynasty.

The book starts with a geographical introduction, in which the author describes what he refers to as Greater Egypt, the length, wealth, and security of which certainly was important to what the ancient Egyptians perceived as their own "manifest destiny." Next comes a historical introduction, which summarizes the two thousand years of history that preceded the reign of Ramses II.

After introducing the new dynasty of military men, Ramses's own family, the author takes us directly through one of the young king's most dramatic moments, the famous battle against the Hittites of Anatolia near the city of Qadesh. Although Ramses, only in his fifth year on the throne, won the day through his obvious bravery, the war resulted in a stalemate, and it was left to the royal propaganda machine to turn the battle into a mighty victory for the king. The long struggle between the two powers resulted in the famous peace treaty between the Egyptians and the Hittites in Ramses's twenty-first year.

Kitchen then takes us through Ramses's peacetime activities. Here we see the king as a mighty builder—erecting new palaces, adding to already established temples as well as building new ones, such as his mortuary temple (the home of Shelley's "Ozymandias") and the famous temple at Abu Simbel.

The book goes on to describe various aspects of life in Egypt under Ramses II, from the layout of the great cities of the time to the official administration of the palace and the activities of its officials, from legal matters to scribal training and belles-lettres. Other sections deal with religious architecture, sacred ritual and festivals, and the everyday life of the workmen who labored on the royal tombs in the famed Valley of the Kings. The book closes with a chapter on the death of Ramses II and includes a useful account of the preparations involved in ensuring the king a successful journey through the underworld. The final chapters touch on Ramses's successors and follow Egypt through its long decline and eventual conquest by other nations.

A description of many of the historical events Kitchen covers can be found in other works, but his narrative really shines where he offers insights into the people of the realm, their desires and goals, virtues and weaknesses, the civil law suits, fiscal scandals, the daily memoranda dealing with the minutiae of government. Through the last fifteen years of collecting all the inscriptional material from the Ramesside period, Kitchen has become the undisputed master of this era, and it is precisely this astonishing command of the sources that has permitted him to write such a clear and vibrant book.

The scholar will find little that is new, save for the usefulness of having all of the material in a single book. But Kitchen did not write his book for the scholar. As he states in his introduction, "This book is offered for enjoyment, not study" (p. vii). For the general reader, however, the book is a delight. Wisely adhering to the old axiom that one never argues the evidence before the readers, Kitchen has told his story in a straightforward manner, and, indeed, his is a tale well told.

RONALD J. LEPROHON
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FRANÇOIS DE POLIGNAC. *La naissance de la cité grecque: Cultes, espace et société viii^e–vii^e siècles avant j. c.* Foreword by CLAUDE MOSSÉ. (Textes à L'Appui.) Paris: La Découverte, with the cooperation of the Centre National des Lettres. 1984. Pp. 187. 96 fr.

In this book François de Polignac examines the contribution of the location of cults to the consolidation of the Greek *polis*. De Polignac is particularly concerned with extraurban sanctuaries, those belonging to the *khora*—the countryside or territory. He rejects the theory that a critical political transition accompanied the substitution of the tutelary divinity of the town for the king, which permitted a division of authority among aristocratic mediators between the god and the community, and argues instead that cities had bipolar (central and peripheral) religious focuses. Accordingly, Athens, with the cult of Athena on the Acropolis, is not considered paradigmatic. This polemic has limited appeal for English-speaking scholars, among whom the hypothesis under attack never won a following.

The first chapter surveys the beginnings of extraurban sanctuaries. Yet the qualifier extraurban is problematical, for it covers rural sanctuaries (such as Aphaia and Perachora) founded before their cities became independent. And Athens is not really a counterexample. The unity of Athenians was symbolized by two festivals, one oligarchic and the other populist, in which the ideological coloration of the unifying hero-king, Theseus, was controversial. The Panathenaia, eventually dominant, served to integrate rather like the festivals examined by de Polignac, but placement had no role in this integration. Peripheral regions of Attica (such as Salamis) were self-evidently late additions, and the superseded eighth-century boundaries could not still symbolize marginality.

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The next chapter, the core of the work, treats the role of homeland sanctuaries in civic consolidation. The approach is anthropological in a limited sense. Theoretical work is not exploited and ethnographic material not adduced. Myth and ritual, however, are examined for evidence about processes of integration. Containing worthwhile observations on the archaeological material, the attempt at a typology of the placement of sanctuaries is valuable. But the author's tendency to conflate all extramural sacred places is disconcerting. Also characteristic is the notion of ritual citizenship—the religious assimilation of political outsiders. Because of conservatism, religious aspects of integration are conspicuous, but de Polignac does not include its material and oral-poetic facets.

In the third chapter the focus shifts to the much-debated peripheral cults of colonies. They are not survivals of contact with the Helladic Aegean or of pre-Greek rites. The author interprets them as a means of claiming territory and of controlling interaction with neighbors. Missing, however, is an inquiry into where symbolic spatial orientation ends and ordinary utilization of land begins. A last chapter on hero-cults (only some of which are extra-urban) is only tenuously connected with the preceding sections, although the significance of archaic "archaeology" in the creation of cults is treated well.

The general focus on the *naissance* of the city is not altogether apposite. Complex rituals show adaptations to subserve evolving social orders. For example, de Polignac knows that the Argive Heraion could represent a sacralization of a boundary or an expression of centrality and regional hegemony, but he fails to fuse political and cultic perspectives to provide a close reading of the sanctuary's history. Nor does he distinguish symbolic actions and actual experiences. Athenian youths enacted an initiatory marginalization and return. Initiations at Sparta were more like rehearsals, since Spartans spent a sojourn "in the brush." Consequently, a peripheral sanctuary was dispensable, since a real retreat from society was implemented. Nevertheless, the author argues that festivals celebrated in Sparta itself are somehow extraurban.

De Polignac presents a creditable first treatment of the placement of sanctuaries for the integration of city-states. Diligent collection of data scattered among archaeological publications is reflected in a useful bibliography. The absence of a genetic perspective on the adaptation of ritual behavior is regrettable; thus, the relationship of archaic cults to other integrating processes still awaits exploration.

THOMAS J. FIGUEIRA
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BOGDAN RUTKOWSKI. *The Cult Places of the Aegean.* New Haven: Yale University Press. 1986. Pp. xix, 267. $35.00.

This book seeks to define cult places by drawing on all the available information: archaeological evidence, pictorial representation, Mycenaean script, auxiliary disciplines, and comparative examples. Beyond cataloguing the data, Bogdan Rutkowski attempts to demonstrate the link between the history of cult places, political and cultural events, and religion. A third aim is to trace the history of research on cult places from Greek antiquity to the present.

As Rutkowski candidly admits, "Of course it has not been possible to achieve all these aims fully" (p. 224). The study is a valuable inventory of archaeological and topographical data of sites regularly identified as cult places, but the discussion of religious views and cult practices in the preclassical Aegean is less than successful. Too much of the chapter examining past research is repeated in the nine chapters devoted to separate categories of cult places.

The review of material evidence is thorough and includes numerous and handsomely reproduced plans, illustrations, tables, and catalogues. Careful attention to the material evidence demonstrates Rutkowski's belief that firm identifications can come only through cult or votive objects as opposed to other kinds of evidence (p. 225). Unfortunately, he sets no firm criteria for the identification of cult places in comparison, for example, with Colin Renfrew's frank discussion of the limitations and value of archaeological data (*The Archaeology of Cult: The Sanctuary at Phylakopi* [1985]). Without anchors of definition, Rutkowski's tone is often tentative and his reasoning circular. Strong and repeated caution about the state of the evidence does not lead to similar reticence about drawing conclusions. Thus, Rutkowski acknowledges that no anthropological cult images predate Late Minoan III, yet he postulates their existence from early in the Bronze Age (p. 228). A huge leap of faith from data to inference is regularly required of the reader. For example, the psychological effects of the eruption of Santorini are held to have occasioned a decline in the old faith and the rise of a new conception of deity as a supreme, universal god in Late Minoan I. It is a surprise to read that we know a great deal about the development of primitive society in Greece and disheartening to find the view, in a 1986 publication, that "these early people were forced to have recourse to those other methods—magic and religion—which in primitive societies take the place of science" (p. 94).

Some of these problems may be ascribed to the fact that the present volume is essentially a second

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edition of *Cult Places in the Aegean World* (1972), an edition that retains most of the earlier study's conclusions (except those concerning temples) but greatly expands the quantity and quality of illustrative data. Some errors have occurred in the revision (MH becomes MM for the mainland), and careful editing should have eliminated typographical and grammatical errors. These problems may disappear when Rutkowski examines the evidence a third time, for he hopes to publish "an extensive study on the problems and methods of the Aegean cult places" (p. 239 n. 83). Rutkowski deserves great credit for setting out the evidence. He and others must continue to question it.

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PETER S. WELLS. *Farms, Villages, and Cities: Commerce and Urban Origins in Late Prehistoric Europe.* Ithaca: Cornell University Press. 1984. Pp. 270. Cloth $32.50, paper $14.95.

Peter S. Wells has written a short but attractive survey of prehistoric Europe during the first millennium B.C., from circa 1000 B.C. (the middle of the Late Bronze Age) to the time of the Roman conquest. A short final chapter, something of an epilogue, covers Romanization and the formation of medieval towns. The account is, of necessity, based almost entirely on archaeological evidence, especially that obtained through the excavation of numerous Bronze Age and Iron Age cemeteries. Special attention is given to the question of commercial and cultural contacts between Central Europe and the Mediterranean world, a theme discussed by Wells in an earlier work, *Culture Contact and Culture Change: Early Iron Age Central Europe and the Mediterranean World* (1980).

Wells argues forcefully that temperate Europe prior to modern times lacked urban centers such as those in the Bronze Age Near East and the Mediterranean world of the classical period and that nothing resembling an urban society developed in Europe before the first century B.C. In late prehistoric Europe few settlements ever had more than from two hundred to four hundred inhabitants and, before 800 B.C., communities with populations over one hundred were virtually unknown. This striking difference has long been a puzzle; for Wells the explanation lies in the very richness and productivity of the European landscape. The Europeans simply had things too good, with most necessary resources readily at hand, no need to practice irrigation agriculture, and no use for the administrative or ceremonial centers that dominated life in ancient Meso-America and the ancient Near East.

This image of late prehistoric Europe as a sort of proto-industrial Garden of Eden dominates Wells's interpretation of the archaeological evidence discussed in this book. He believes that slavery was unknown in Bronze Age and Iron Age Europe and that the cemetery associated with the community that worked the Hallstatt salt mines during the period circa 800–600 B.C. has revealed no great variation in wealth between burials. Wells claims that "the distribution of grave goods implies that the work was profitable for everyone engaged in the venture" (p. 98). This in spite of Wells's assertion that the period was marked by the rise of entrepreneurs "who perceived the possibilities of gain in the industrial production of salt and iron and who had at their disposal the wealth with which to finance the initial stages of extraction" (p. 98). One can only conclude that such entrepreneurs lived neither at Hallstatt nor at the iron-production center of Stična.

Wells's historical interpretations tend to be rather simplistic, even naive. This is nowhere more apparent than in his treatment of the complex problems associated with the still poorly understood transition between the Hallstatt Early Iron Age and the La Tène Late Iron Age, a transition played out during the course of the fifth century B.C. and traditionally connected with the rise of the Celts, who were to dominate the European scene down to the time of the Roman conquests.

Wells accepts at face value the explanations by such classical authors as Dionysius of Halicarnassus and Livy that have long been recognized as little more than a string of literary topoi. Wells does not discuss the social tensions of late Hallstatt Europe, which resulted in the destruction of the fortified *Fürstensitze* with their wealth of southern imports and in the migration south of large groups of men, women, and children in search of a more stable society.

Wells provides an excellent summary of the results from the excavations at many of the important sites of late prehistoric Europe and gives valuable discussions of the development of the technologies of salt extraction and iron production, the two main industries of Europe during the first millennium B.C. For an understanding of the social and economic forces that shaped Europe during this period and resulted in what has been called "the crisis of the fifth century," those seeking a synthesis in English should consult *Settlement and Society: Aspects of West European Prehistory in the First Millennium B.C.*, a collection of essays edited by T. C. Champion and J. V. S. Megaw (1985), which includes a study by Wells on Mediterranean trade and culture change in Early Iron Age Central Europe.

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MEDIEVAL

HEINRICH FICHTENAU. *Lebensordnungen des 10. Jahrhunderts: Studien über Denkart und Existenz im einstigen Karolingerreich.* (Monographien zur Geschichte des Mittelalters, number 30, parts 1 and 2.) Stuttgart: Anton Hiersemann. 1984. Pp. ix, 323; vi, 327–614. DM 396.

This is an extraordinary book, and what is really surprising is that it was written by an Austrian specialist in diplomatics rather than an *Annaliste*. Heinrich Fichtenau, who is probably best known among English-speaking readers for *The Carolingian Empire: The Age of Charlemagne* (1957), describes the mental world of the tenth and early eleventh centuries. He is not concerned with political developments, or even with social history as such, but rather with the beliefs and customs that shaped daily existence and provided a degree of order in a chaotic world. Although Georges Duby examined in his *Les trois ordres ou l'imaginaire du féodalisme* (1978) the origins and evolution of a theoretical construct, the tripartite division of society, Fichtenau focuses on the groups and institutions that actually existed and, more important, contemporaries' perceptions of them.

The chief characteristics of the age were an inability to penetrate beyond external appearances or to think in abstract terms. We are inclined, Fichtenau points out, to perceive the post-Carolingian world as monolithic, but its inhabitants were confronted with conflicting value systems and principles of organization. For example, clerical celibacy was difficult to enforce in a society in which family ties were the chief social bond. Western society has changed greatly during the last millennium, but Fichtenau maintains that there is also considerable continuity between Carolingian Europe, the tenth century, and our own era. Although we may no longer think that seating arrangements replicate the celestial hierarchy, the annual official portraits of the justices of the Supreme Court show that such concerns with rank are not totally alien to modern society.

Fichtenau examines six major topics: order, the family (in its classical and modern meanings), the nobility, the religious life, the common people, and disorder. Each of these is divided into several subsections. For example, he deals in the section on the nobility with the concept of nobility, the king, the bishop, and the secular clergy. A mere listing does not do justice, however, to the content. Fichtenau discusses and analyzes such disparate topics as the role of women, family strategies, monastic customs, relics, ploughs, diet, weeping, heating systems, toilet facilities, and chess. There are nuggets on nearly every page: a cleric shaved to show that he had become a "new man" (p. 100); hermits were the antithesis of the nobility, whose life style they rejected, but shared the nobility's consciousness of belonging to an elite (p. 329); and a horseshoe became a symbol of luck in a society where iron was so scarce that horseshoes doubled the price of a horse (p. 443). All of this is based on a vast knowledge of the sources, which Fichtenau subjects to the "close reading" techniques of modern anthropology, and an equally impressive command of the secondary literature. The book reminded me most of Johan Huizinga's *Waning of the Middle Ages*, albeit without the artistic evidence, and there is the danger that it too will merely be mined for its examples.

Such a tour de force will inevitably arouse some second thoughts. It is possible to question some of Fichtenau's conclusions. He observes that we know virtually nothing about the role women played in the naming of their children, but we can suspect some wifely influence if the maternal ancestry was emphasized. Fichtenau cites as proof Louis d'Outremer, who named two of his children after his wife's parents (p. 129), but surely it did not take much persuasion to name one's children after the German king and queen. Second, we see the tenth century, as Fichtenau realizes, largely through the eyes of the clergy. This is a serious problem in a book that attempts to analyze the *mentalité* of all strata of society. Let me cite a simple example. Fichtenau points out that great importance was attached to sitting on the right hand of the lord but that this custom, which is mentioned in the Bible, occurs in other societies as well (pp. 44–45). Even if we assume that such a custom was indigenous to the West and was not inspired by the Gospels, how do we know that scribes did not record these disputes about seating arrangements and exaggerate their significance on account of the biblical parallels? Finally, although Fichtenau does mention regional differences (for instance, in the building of castles), the emphasis falls on the common features of European culture. This is a valuable corrective to political histories of the tenth century that have dealt with the dissolution of the Carolingian empire and the emergence of the successor kingdoms, but citing examples from all over the Continent may blur the distinctions too much, particularly for the unwary reader. The book should be translated (and preferably sold at a considerably reduced price) to make it more accessible to English readers.

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PATRICK J. GEARY. *Aristocracy in Provence: The Rhône Basin at the Dawn of the Carolingian Age.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1985. Pp. ix, 176. $29.95.

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For years European historians have been trying to describe with precision the social changes that occurred during the seventh and eighth centuries, especially as regards the origin and composition of the nobility. Patrick J. Geary has plunged into this debate, using as a vehicle the testament of Abbo, an important figure in the aristocracy of northern Provence, who was, by 739, a trusted follower of the Carolingian Charles Martel.

Abbo's testament is a detailed and informative document that German and Italian scholars have been reluctant to use as a source because it has come down to us in a forged Carolingian diploma and was of questionable authenticity. Geary has made painstaking and generally persuasive arguments to show that the will itself is authentic and thus a valid source for the social and political conditions in early eighth-century Provence. I remain somewhat uneasy about its use of Roman legal forms that seem appropriate to an earlier or later period but a bit sophisticated for the eighth century, yet Abbo's region did have probably the most Romanized legal tradition in France.

Having argued convincingly for the authenticity of the testament, Geary then edits the document, correcting minor errors by six previous editors and providing a translation into English. The key element in this process is his choice of the English words into which to translate certain technical terms describing landholdings and the status of individuals. In subsequent chapters he justifies the terminology he has chosen. I find his decisions eminently sound, except for one that has no bearing on his scholarly argument. Unwilling to accept "freedmen" as an entirely gender-free rendering of *liberti*, he feels obliged to cater to twentieth-century sensibilities and introduce the awkward term "freedpersons," thereby giving his translation a curiously anachronistic flavor.

Analyzing Abbo's testament, Geary demonstrates that "the nature of properties and the conditions of the persons holding, inhabiting, and working these properties vary widely in the region in the early eighth century" (p. 99). For those who teach this period, to make generalizations about rural society will be harder than ever, but to have all the variations presented so clearly in a case study is splendid.

In his last two chapters Geary takes up the complex question of aristocratic ethnicity and kinship. It seems that Abbo and his peers had both Gallo-Romans and Franks among their forebears and their political connections, but, if the two ethnic groups had, in this sense, fused, an ethnic consciousness remained. This consciousness was, however, subjective and fluid, bearing little relationship to such objective criteria for ethnicity as blood, language, or law. The Provençal "revolt" of the 720s and 730s was not a struggle of Gallo-Romans against Germanic control but, rather, an extension of the struggle, thoughout the Frankish world, between the party of the Neustrian mayors of the palace and those who backed the upstart Arnulfing (Carolingian) family that was seizing power.

This book is an outstanding piece of scholarship that contributes greatly to our understanding of this complex phase of early medieval history.

JOHN BELL HENNEMAN, JR.
Princeton University Library

JOHN B. FREED. *The Counts of Falkenstein: Noble Self-Consciousness in Twelfth-Century Germany*. (Transactions of the American Philosophical Society, number 74, part 6.) Philadelphia: The Society. 1984. Pp. 70. $10.00.

KARL SCHMID. *Gebetsgedenken und adliges Selbstverständnis im Mittelalter: Ausgewählte Beiträge; Festgabe zu seinem sechzigsten Geburtstag*. Sigmaringen: Jan Thorbecke. 1983. Pp. xiv, 652. DM 120.

Karl Schmid is arguably the contemporary German social historian whose work has had the greatest impact on medievalists in the English-speaking world. Briefly stated, the "Schmid thesis" is that, prior to the eleventh century, European aristocratic kindreds were organized not into agnate lineages but rather into wider clans that included both agnates and cognates. The transformation of these clans into narrower lineages was one of the major social and cultural transformations of the Middle Ages. Yet Schmid's contribution to social history is far greater than merely the Schmid thesis, and this collection of his most important essays challenges historians to come to terms with the considerable achievements of this scholar.

The volume includes twenty essays grouped thematically into four interrelated areas. The first focuses on the role of individuals and collectivities and explores, through specific examples, how best to examine the relationship of person to group in this society. The second deals with the structure of the early medieval aristocracy and includes the two seminal articles for which Schmid is best known. The third, which contains some of Schmid's finest work, considers the self-perception of aristocratic kindreds. The study of Welf self-perception is perhaps the finest study of aristocratic *mentalité* ever written. The fourth treats liturgical remembrance and focuses on Schmid's most controversial research: the exploitation of *libri memoriales* as evidence of aristocratic self-perceptions.

Schmid's great and enduring contribution in these essays is not his discoveries about medieval society but rather his elaboration of a process by which medieval society should be discovered. He rejects the most traditional approaches: genealogical

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and regional reconstructions. The dangers of the first, he argues, are twofold. First, all too frequently the data are contradictory and incomplete. As he shows in the case of Count Kuno of Öhingen, when taken as a whole the elaborate attempts at genealogical reconstruction often simply cancel each other. The second and even more significant problem is that, even if one can reconstruct genealogical schemas, one cannot conclude that the biological relationships thus discovered had any meaning for the individuals themselves. Genealogy, as he says in his discussion of nobles and reform in Swabia (p. 344), is not a historical reality until it is self-consious. The second approach, the regional study, presupposes that social structures are essentially regional and thus ignores transregional groups. Schmid argues that both approaches inappropriately impose anachronistic structures and categories on the past. One must begin rather with those sources in which medieval groups identified themselves and then explore the meanings of these self-identifications. The search for such self-expressions of group solidarity led Schmid to a wide variety of contemporary sources including contemporary *genealogiae*, chronicle references to kinship charter evidence, and, most important but never exclusively, *libri memoriales*, into which were entered the names of identifiable groups of medieval aristocrats. From these contemporary expressions of group identity Schmid proceeds to reconstruct the complex networks that composed perceived kindreds. Furthermore, he examines their disintegration into agnate-centered houses in relationship to the increasingly hereditary nature of office and to the establishment of hereditary advocacy of reformed monasteries. The result is a history that destroys anachronistic distinctions between lay and monastic society, affirms the liturgical nature of medieval society in general, and anchors imperial politics and movements such as the Gregorian reform firmly within a dynamic social process. As a whole, this collection challenges historians of any region and period to begin with contemporary models of group identity and self-perception and then to go on to rediscover the society in which they had meaning.

The one American scholar who has applied Schmid's method in a serious and productive manner is John B. Freed, whose study of the counts of Falkenstein is a meticulous and fruitful application of Schmid's approach to noble self-perception. Between 1166 and 1200 Count Siboto IV compiled a record of his family's power, wealth, and identity. This so-called *Codex Falkensteinensis* is the earliest example of an aristocratic archive and provides Freed with the ideal contemporary document through which to examine the self-identity and familial perceptions of an extraordinary German noble.

Freed juxtaposes the material compiled by Siboto with other independent sources in order to elucidate the fascinating process by which Siboto created his family identity through a careful selection of genealogical material and provided for the continuity of the family by pursuing a strategy of marriages and manipulations of family property. Freed further investigates the perceptions of kinship through an excellent discussion of the family's naming traditions. Finally, he considers the extinction of the family in the thirteenth century, a process he attributes to the same values and strategies that had created it.

The most remarkable aspect of this study is the astute combination of Freed's detailed knowledge of the sources and literature on south German aristocracy with a broad, comparative European perspective. Its value is marred by two weaknesses. First, one has difficulty imagining its intended audience: the author seems to expect that the reader knows the *Codex Falkensteinensis* and the history of Austrian aristocratic families as well as he does. Second, the book is so compressed that it is almost an outline. Sixty-seven pages are too few within which to do justice either to Siboto's self-perceptions or to Freed's learning.

PATRICK J. GEARY
University of Florida

ERNST KARPF. *Herrscherlegitimation und Reichsbegriff in der ottonischen Geschichtsschreibung des 10. Jahrhunderts.* (Historische Forschungen, number 10.) Stuttgart: Franz Steiner. 1985. Pp. vi, 237.

This is a comparative study of tenth-century historiography, specifically, the views of writers such as Liudprand of Cremona, Hrotsvit of Gandersheim, Widukind of Corvey, Flodoard of Reims, and the authors of several saints' lives on the accession of the first king of the Saxon house in 919 and the imperial coronation of his son in 962, on the legitimate roots of royal and imperial power, and on the idea of empire in tenth-century Germany. Ernst Karpf painstakingly examines sixteen historiographical works and determines their stance on the issues raised by the events of 919 and 962. The historical sources that bear contemporary witness to the rise of the Saxon royal dynasty and the emergence of the German state have been subjected to microscopic inspection before. As a result, the author gives considerable attention to a review of earlier scholarship, especially in the cases of Liudprand and Widukind, much of it carried on in the book's critical apparatus, which fills as much space as the text of the book itself.

The texts in question varied greatly in geographical origin, context, purpose, and historical genre.

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Accordingly, their comments on kingship and empire also differed significantly. The transfer of the crown to a new dynasty in 919 sharpened the issue of royal succession and was justified by ideas that by that time had developed in early medieval political theory, including the right of inheritance, the ruler's designation of his successor, and the election of the king by princes and people. The hagiographical sources, whose focus was on their heroes or on specific churches, emphasized the constitutive act of priestly consecration of the ruler, which embraced not only the idea of ecclesiastical participation in the making of the king but also the notions of divine appointment and of suitability for the office as determined by the church. In the making of the emperor, the role of the pope was not easily ignored, but rule over more than one kingdom or a great military victory, such as the defeat of the Hungarians on the Lechfeld in 955, could also be claimed as sufficient conditions.

In a final section Karpf attempts to arrive at some general conclusions. All texts examined saw the accession to the throne of the Saxon royal family as a new beginning, although they differed on the degree of change or continuity that the event represented. Some placed more, others less emphasis on the involvement of the royal family in governance, but all agreed that the king himself stood out as the undisputed leader of that collective entity. All texts that mentioned it accepted the transcendent significance of the coronation in 962, but Widukind and a few others maintained that the imperial character of Otto I's kingship predated that event. The sources imply that Otto's subjects were seen as a "people of empire" but also that the empire was considered by others as no more than the sum of the territories governed by the Saxon kings. Such conclusions suggest that no consistent answer to the questions raised by the author emerges from this study of Ottonian historiography, and one must see the merit of this book in the meticulous analysis of individual texts.

BERNHARD W. SCHOLZ
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RALPH V. TURNER. *The English Judiciary in the Age of Glanvill and Bracton, c. 1176–1239.* (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1985. Pp. xiv, 321. $44.50.

In this study Ralph V. Turner attempts for the first time a composite portrait of the judges who played the largest role in applying English common law during the half century after Henry II's reforms. In sections devoted to each of four kings, from the last decade of Henry II through the first twenty years of Henry III's reign, Turner has found, from among some three hundred men who served, however briefly, on eyre, at Westminster, or in the court *coram rege*, a group of forty-nine whose careers were long enough to provide substantial answers to the questions he wanted to ask.

His central question concerns the origins of the "professional judiciary" that came into view after 1194, when Hubert Walter regularized and organized the courts as had no other justiciar before him. Most of the earlier judges had been multipurpose royal servants, magnates, and high-ranking royal officers who shifted between different areas of government work. The professional judiciary was composed of men who during their careers devoted themselves exclusively to judicial work. This process was completed in roughly the first decade of Henry III's reign, and the careers of Martin Pattishall and William Raleigh, who rose through judicial clerkships to judgeships, exemplify that event, although not all followed that particular route.

Beyond this central theme Turner systematically addresses the social and family backgrounds and patronage connections of his forty-nine subjects, their career development, and the rewards received for or through their work. They were mostly of knightly families and were equally clerical and lay. Some but not all accumulated considerable wealth through favor or through patronage, which often hinged on their professional work.

In one appendix is a schedule that orders certain kinds of information for Turner's selected group. Additional schedules and lists would increase the usefulness of the large amount of historical data presented in the text, especially if the schedules treated most of the large group of three hundred judges to see what patterns emerge among so many who had brief judicial careers. Additional categories of information in such lists would have enlarged the value of the work.

Among the significant mechanical faults are inconsistencies in the footnoting format, which annoy and hinder in the absence of a bibliography. Some items in the index are misplaced, and, in addition to other spelling errors, the name of Henry de Pont-Audemer and his village is misspelled on sixteen occasions. All of this speaks of less than perfect proofreading and editing.

In conclusion, it should be said that the "Age of Bracton" at one time referred to the most famous legal author of medieval England and not to the book that he was thought to have written during the 1250s, toward the end of his career and life. By using the "Age of Glanvill and Bracton" and ending his study at 1239, Turner employs for the first time in the schemata of a book-length work Samuel Thorne's discovery that Henry Bracton (or Bratton) was not the sole author of the great law tract formerly attributed to him but that several persons,

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foremost among them William Raleigh, compiled the greater part of that treatise before 1239, when Raleigh left his judicial career for a bishopric and when Bracton was still a judicial clerk awaiting his appointment to a judicial commission.

FRANKLIN J. PEGUES
Ohio State University

JANE E. SAYERS. *Papal Government and England during the Pontificate of Honorius III, 1216–1227*. (Cambridge Studies in Medieval Life and Thought, third series, number 21.) New York: Cambridge University Press. 1984. Pp. xv, 292. $49.50.

Jane E. Sayers's book is welcome on at least two counts. Those who study the mechanisms by which the papal chancery operated in the thirteenth century will find valuable the analyses of Honorius's chancery and its products, including the papal registers and Honorius's surviving letters. Appendix 2, in fact, provides a calendar and diplomatic description of fifty-seven letters that have survived in original form and have an English interest; twenty-seven of them have never been printed and are edited herewith. But this book is not only for those concerned with thirteenth-century canon law and diplomatics. Sayers also treats the interpretation and application of papal politics in England during the first third of that century. King John surrendered his kingdom to Pope Innocent III in 1213 to be held as a papal fief. The papacy thus was afforded important but definite powers and responsibilities. The book's second part, and especially its final chapter on curial relations with England, is devoted to a discussion of the production and preservation of thirteenth-century papal canon law and the implications of the special situation created by John and Innocent.

"Was Honorius a continuator or an innovator?" Sayers asks in the first sentence of the epilogue. The evidence suggests the former. "No pope by the early thirteenth century could divorce himself from the effects of his predecessor's rule" (p. 194)—particularly, one might add, when the preceding pontificate was the eighteen-year reign of Innocent III. The precedents, the Fourth Lateran Council, and especially the growth of papal administration made such a disjuncture impossible. Honorius's pontificate is a link, and "by no means a weak link" (p. 194), in the chain between Innocent III and Gregory IX.

Many features of this carefully prepared volume are laudatory, but of special note is the lucid presentation of details about the operations of the Roman church, and especially the papal chancery, in the twelfth and thirteenth centuries. Extending for more than one hundred pages, that section incorporates both Sayers's own research and a thorough knowledge of Continental scholarship and assembles material from a variety of sources that are not always easily accessible. Furthermore, Sayers's views about the purpose and method of compilation of the official canon law collection put together during the pontificate of Honorius—the so-called *Compilatio quinta*, promulgated in 1226—will be of great interest to those concerned with the development of such books from the days of Innocent III and the *Compilatio tertia* down to the *Decretales* of Gregory IX. Sayers includes two extended appendixes: first, a list of those scribes who copied Honorius's original letters concerned with English topics and those scribes who worked during Honorius's pontificate and are discernible from other sources; second, the aforementioned calendar and partial edition. In sum, this book, rich in information and opinions, should be consulted by anyone who labors to comprehend the papacy and England in the first half of the thirteenth century.

ROBERT SOMERVILLE
Columbia University

JÜRGEN PETERSOHN. *Ein Diplomat des Quattrocento: Angelo Geraldini (1422–1486)*. Tübingen: Max Niemeyer. 1985. Pp. xviii, 384. DM 104.

Angelo Geraldini, bishop of Sessa, was a clerical careerist who for more than forty years served seven popes. A doctor of pontifical law, and, as Jürgen Petersohn depicts him, a harsh, jejune, humorless bureaucrat, he started in the papal chancellery as abbreviator and advanced to secretary and protonotary. As a military governor of regained townships and as a prosecutor, he was used repeatedly in the popes' warfare to reconquer ecclesiastic territory lost to Rome's neighbors. At times he was sent abroad on state business regarding the ever-shifting alliances between the powers confronting, or associating with, the papacy: Milan, France, Naples, Florence, Venice, Genoa, and Spain. But Geraldini's missions were seldom successful for he was not equipped with the rank and full powers of a nuncio, and he lacked diplomatic skill. The greater part of this biography consists of accounts of these events, which the author depicts one-dimensionally. One misses short profiles of the various rulers—dukes, kings, statesmen—who received the bishop. The story remains rather lifeless, buried under excessively long comments on, and analyses of, Geraldini's usually erroneous assumptions of his own success. The work gains interest only when Bishop Angelo, at sixty, is sent on an urgent mission to Basel, where, at last, a palpable confrontation occurs. But, even in the three long chapters dealing with this case, Petersohn does not tell enough about Geraldini's opponent, the rebellious Cardinal

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Andreas Jamometic, Archbishop of Krain (now Kranj, Yugoslavia), an offender against the church.

In 1483, the year of Martin Luther's birth, Geraldini was in the midst of a two-year struggle with Basel to obtain Jamometic's extradition. The rebel, who was the town's guest under safe-conduct, had fled there, denouncing the corruption of Rome, calling for a new council, and demanding the deposition of the pope. Jamometic had gone to Rome as envoy of Emperor Frederick III to receive his cardinalate from Pope Sixtus IV. But the new cardinal, already in possession of his two red hats and shocked at the conditions prevailing in the city, caused such an uproar that he was imprisoned in Castel Sant'Angelo, from which he escaped. Sixtus dispatched Geraldini in pursuit of the fugitive with orders to bring him back. When the Baselers refused to seize the cardinal, Geraldini poured down on them all the torrents of anathema and put the town under interdict, swearing to destroy the city to make it disappear like Numantia and Carthago. In fact, he applied economic sanctions by also threatening with excommunication everyone beyond the walls still trading with Basel. Finally, Frederick III consented to the rebel's incarceration, but he forbade his extradition. Forced to leave behind the pre-Lutheran reformer, Geraldini returned from this most important mission of his life also in disappointment. This case drew the attention of Jacob Burckhardt, who in an essay (1852) called Geraldini "a bishop well known for his malice" and "a legate full of tricks" (p. 216).

Angelo Geraldini had risen in the papal court from the poverty of his childhood in Amelia (Umbria) to wealth, but he never reached the goal of his obsession, which was, as Petersohn explains, the cardinalate, a rank for which he lacked the qualifications of personality, family background, and princely connections. This book is well researched, but it is regrettable that the author has chosen so inconsequential a historical figure as subject for his work of solid scholarship.

ERIKA SPIVAKOVSKY
Westport, Connecticut

ELLEN WEDEMEYER MOORE. *The Fairs of Medieval England: An Introductory Study*. (Studies and Texts, number 2.) Toronto: Pontifical Institute of Mediaeval Studies. 1985. Pp. x, 401. $34.00.

The subtitle of this book by Ellen Wedemeyer Moore is too modest to represent the depth of research and the wealth of information it contains. Although only the fairs at St. Ives and at Winchester are studied in detail, the information about these fairs is indeed extensive; the author discusses the types of merchandise sold, how merchants from Flanders and from various parts of England operated, and the rules and general administrative procedures followed at a fair. Part 2 of the book consists of a study of St. Ives as a rural town dominated by the annual fair.

Most historians will probably use this volume selectively as a handbook about fairs rather than read it from cover to cover. For example, a historian interested in royal purchases or prizes could profit from certain sections. In some ways the book is reminiscent of the type of economic history pioneered early in this century by scholars such as William Cunningham, Norman S. B. Gras, Charles Gross, Ephraim Lipson, and Louis E. Salzman. This study, however, also uses the insights gained from recent historical investigations carried out by J. Ambrose Raftis and others at Toronto. The result of this blending of the older scholarly interest in the economic institution of the medieval fair with newer historical questions is a fuller, richer study than those produced by the pioneers in economic history.

As a narrative history of English fairs this book provides only an introduction. Fourteen pages describe the height of prosperity of fairs between 1180 and 1220 in relation to the woolen textile trade, eighteen pages explain the decline of fairs, and twelve pages are devoted to a comparison with the Champagne fairs. Although the author believes it is premature to attempt extensive generalizations, some general indications emerge from her work more as suggestions than as fully developed conclusions. The examination of St. Ives as a rural town organized as a manor but functioning as a commercial center raises again the question whether historians should make a sharp distinction between a town and a village, between urban and rural. With peasants taking an active part in the commerce of the fairs, especially at St. Ives, the view of the peasant as restricted to a passive and oppressed role in society is called into question. A new perspective on the highest ecclesiastical positions is given when the author emphasizes the business skills with which the bishop of Winchester and the abbot of Ramsey managed their respective fairs. Another line of inquiry that might profitably be extended is the strong case that the credit policies of the English kings were detrimental to the fairs and, finally, were important in the decline of the institution.

Because fairs were places where almost all groups of medieval society met and interacted, the author suggests that the history of fairs has broader implications for social history. Perhaps other scholars will try to answer some of these questions, but most historians will refer to this book as a convenient storehouse of information about the fair as it flourished temporarily in medieval England.

CHARLES R. YOUNG
Duke University

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P. D. A. HARVEY, editor. *The Peasant Land Market in Medieval England*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xvi, 375. $47.50.

Ever since M. M. Postan and C. N. L. Brooke published the *Carte Nativorum* in 1960, historians of medieval English rural society have been aware of the importance of the traffic in land to English peasants. This book, edited by P. D. A. Harvey, finally gives to specialists and nonspecialists alike a collection of intensive studies of this phenomenon as revealed by local sources from four separate regions in England: Janet Williamson writes on thirteenth-century Norfolk, Rosamond Faith on fourteenth- and fifteenth-century Berkshire, Andrew Jones on fifteenth- century Bedfordshire, and Tim Lomas on fourteenth- and fifteenth-century Durham.

Relying especially on evidence recorded in village court rolls, the authors present detailed analyses of the scope of the land market and its exploitation by individual peasants and peasant families in the late Middle Ages. Throughout the four studies, careful attention is given to the impact of population growth and decline and inheritance customs on late medieval English land hunger and surplus and the strategies employed by peasants either to increase their properties beyond a bare subsistence level or to rid themselves of holdings under a subsistence level. Of special value is the evidence presented of the extent to which peasants manipulated the land market to supplement or even by-pass the inheritance practices of customary law. In addition, the four studies serve as powerful demonstrations (still necessary, even today) that beneath the rigid pictures of tenure given by traditional manorial records existed a multilayered and exceptionally fluid network of individual relationships to land. Perhaps most important, these studies emphasize the variety of medieval peasant experience, a variety that owes much to the individuals involved, their personal resources and resourcefulness. The essays remind us that, when all is said and done, there was no such thing as the "peasant" but rather a broad spectrum of medieval "peasants."

The four studies are all examples of solid, painstaking, and rigid scholarship, respectful of both the limitations and possibilities of the sources and valuable as demonstrations of the crucial importance of court rolls in exploring the realities of rural life. In addition, they are bracketed between two strong essays by the editor—the introduction and the conclusion—which do more than comment on the individual studies: they build on them, skillfully pointing to further questions for investigation. This is, in short, a book that serves two basic purposes: to present what is known about the peasant land market and to suggest future lines of research. One could not ask for much more.

One could not complain about the contents of this volume; Harvey is to be praised for bringing the work of these four scholars to the printed page. It is, however, a sobering commentary on current trends in scholarly publishing that it has taken so long for these studies to see the light of day. All were originally doctoral theses, which have been shortened and reworked. But the time between completion of the thesis and appearance has ranged from ten to twenty-four years. This is deplorable. We are fortunate to have these essays at long last, yet one cannot help but wonder; if work on this quality has been hidden for so long, what else remains out in academic limbo looking for a home?

EDWIN B. DEWINDT
University of Detroit

JOHN STEANE. *The Archaeology of Medieval England and Wales*. Athens: Univerity of Georgia Press. 1984. Pp. xvi, 302. $27.50.

This book is a late twentieth-century version of Peter Quennell's series on everyday life in Britain, with more scholarly apparatus and less elegance of style. It is not a history of the development of archaeology in Britain or a gazetteer of medieval archaeological sites, nor is it a collection of regional surveys and comparisons or a history of the development of material technology in the Middle Ages. Rather, we are presented with an examination of those aspects of human activity that give rise to material remains: buildings in all their various functions, communications, field systems, industrial enterprises, and, finally, those smaller items of domestic life such as cooking pots and pruning knives. John Steane sets each of his chosen artifacts into its social context and is generous in the breadth and depth of background provided, crossing the boundary from archaeology into social history. His readership is the general public, that multitude that visits museums and wants to know why the objects on display were constructed and how they worked. Steane's erudition is impressive. The book has no bibliography, but the extensive footnotes to each chapter serve the same purpose and will be of considerable use to the professional historian as well as to the lay reader desirous of exploring the subject at greater depth.

In deference to his readership, the author has been at pains to transform the laconic style of the field archaeologist into the more graceful and witty style of the Hoskins tradition. The attempt, though appreciated, has not been altogether successful, and many infelicities remain. At times whole sections and paragraphs have been drastically pruned in the interests of economy, and the text becomes difficult to follow. On several occasions the sense has been

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altogether lost, as in the following example: "Viewed from the air large parts of central and southern England were until recently largely eliminated by modern farming techniques, covered with Ridge and furrow . . ." (p. 153). Such sentences abound, the sense lost by hacking the text, and should have been spotted by the copyeditor, while the editor could have removed the numerous asides with exclamation marks directed at juvenile readers.

Given the enormous scope of the subject matter, inaccuracies of fact or interpretation are creditably few. Since the author has assumed throughout that regional differences are not important (there is not a single map in the book), it hardly matters that Wales received only two or three mentions. When it comes to field systems, however, regional differences are of central importance, and here Steane's work is at its weakest. The vast literature on the subject,—from H. C. Bowen's *Ancient Fields* (1962) and H. L. Gray's *English Field Systems* (1915) through to A. R. H. Baker's *Studies of Field Systems of the British Isles* (1973) and the relevant volumes of *The Agrarian History of England and Wales* and the *Agricultural History Review*, not to mention the *Making of the English Landscape* series—receives no mention, even in the footnotes. The historical geographer will feel disappointed.

Historians too will be disappointed when they read that the Black Death had no long-term effects because the owners of large estates suffered no permanent drop in revenue, a line of reasoning that overlooks the theme of the book itself, namely, the history of tangible things other than money. Because land was successfully switched from arable to pasture, landlords were able to recoup their losses, ridge and furrow became fossilized under grass, the smallholding gave place to the yeoman farm, and large tracts of land reverted to forest. Men took to eating beef and mutton on a daily or weekly basis, consuming more dairy products and more beer, and producing more bacon on the by-products of dairy and brewery. Similarly, although he has much of interest to report on climatic changes, the author makes no attempt to tie in the results with demographic factors. I must also mention a small slip of the pen: textile production in the thirteenth century shifted not from southeast to northwest but to southwest (p. 171).

The book is well bound and pleasingly presented. Illustrations in the form of photographs and archaeological drawings are abundant but are not always placed reasonably close to the relevant text. Many of the drawings are reproduced from books and journals and contain information extraneous to the text (for example, figure 5.7) or are not discussed in the text at all, and readers are expected to make what they will of them. Better use of keys and captions could have removed this irritant.

Although the publisher's constraints are evident throughout the book, it is nevertheless a valuable synthesis of archaeological achievements in the past few decades and will find a place on the shelves of the educated public alongside the publications of historical geographers and local historians interested in this popular field of social history.

CICELY HOWELL
Australian National University

RICHARD J. GOY. *Chioggia and the Villages of the Venetian Lagoon: Studies in Urban History*. New York: Cambridge University Press. 1985. Pp. xvii, 349. $54.50.

Several historians, including Saverio Muratori and Paolo Maretto, have studied the way the islets of Venice acquired their urban forms. What they could learn about the process was, however, limited because central Venice was fully built up during the eleventh and twelfth centuries and no accurate maps or panoramic drawings from that time have survived.

Richard J. Goy is both a practicing architect and a trained historian, and in most of this interesting book he deals with the urban formation not of the city but of the widely scattered islands in the Venetian lagoon. Yet he also has his eyes on Venice. By retracing the story of the formation or development of the lagoon islands in the late medieval and early modern centuries, for which sources are more abundant, Goy attempts to shed light on the earlier development of the islets of the city.

To those who know the surroundings of Venice, the most familiar of the islands Goy deals with will be Murano, known for its glass industry; Burano, a picturesque fishing village; and Torcello, a desolate place whose lonely cathedral holds magnificent mosaics. Chioggia, at the southwestern end of the lagoon, is the major fishing port of the Adriatic. Most of the other islands are narrow sandbars with elongated villages of fishermen and market gardeners.

Goy shows that the first islanders built their huts or houses along the sides of the natural lagoon channels that were essential to their livelihoods as fishermen or market gardeners. This settlement pattern, he says, accounts for the communities' linearity. This finding is no surprise. He also shows that after the initial settlement each community went through the same subsequent phases of urban formation. Typically, the end result was, appropriately enough, a street pattern like the spine and ribs of a fish.

Goy writes clearly and provides splendid maps and illustrations, but his recounting of this story of urban shaping is a little tedious and repetitious,

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because he tells in turn the story of each of more than a dozen islands. Along with his story of how the communities physically took shape, he mixes in a good deal of social history, explaining how the islanders struggled first with plague and later with overpopulation, lack of potable water, silting canals, and malaria. The social history is often interesting, but it obscures somewhat the outlines of the urban formation story. From time to time his readers may wonder if they have misunderstood what goal they are slogging toward.

Near the end, however, Goy does wrap everything together. Linking up his findings to those of Muratori, he argues that Venice itself must have taken shape in much the same way that the communities in the lagoon later did. Houses grew up first on the banks of the Grand Canal and other important canals. Then the spine-and-rib street pattern developed on each islet behind the initial houses, with a church and square in the middle of the islet or along one side.

Although scraps of physical evidence indicate that the islets of Venice did indeed develop in this way, Goy's argument about the formation of Venice obviously depends heavily on the use of historical analogy. What he does with analogy is something like what anthropologists now call "upstreaming."

Goy's reconstruction of the story of the urban shaping of the lagoon islands, which fills nine-tenths of the book, seems to be very sound. His argument about what the formation of the islands reveals about the formation of Venice itself is in the final analysis unprovable but very plausible.

JAMES C. DAVIS
University of Pennsylvania

JANNIS WILLEM MARSILJE. *Het financiële beleid van Leiden in de laat-Beierse en Bourgondische periode, ±1390–1477* [The Financial Administration of Leiden in the Late Bavarian and Burgundian Periods, ±1390–1477]. (Studies over Holland in de Meddeleeuwen, number 1.) Hilversum, the Netherlands: Verloren. 1985. Pp. 347.

Leiden's urban development was not precocious; its aldermen bought their offices from the counts of Holland until 1434. Municipal financial administration was the legal responsibility of four burgomasters, but in periods of stress or party strife they were joined by special captains or treasurers. Particularly after a notable expansion of the city bureaucracy about 1450, the burgomasters generally left the mundane details of collection and disbursement to the city clerks and confined themselves to formulating policy.

Although Leiden experimented with direct taxation in the early fifteenth century, it avoided this during the Burgundian period. Farmed excises on consumer goods, particularly beer, provided most of the city's income. The tax farmers often paid the city's creditors directly without going through the city administration, since some sources of income were earmarked for particular categories of expenditure. Indeed, an outstanding merit of Jannis Willem Marsilje's book is his willingness to examine the often-chaotic reality behind the regularity of the formal accounts. Curiously, the city corporation only began auditing the accounts in 1449 and the counts of Holland in 1500.

A central, if unstated, theme of this book is that Leiden's finances were essentially sound as long as the city government was left to itself. But the demands of the counts of Holland for aid, particularly during the 1420s and between 1465 and 1477, imposed strains that could only be met by the sale of life rents. The accounts of Leiden show a generally negative balance until the advent of the Burgundian dynasty in Holland in 1434. The ordinary revenues of the city generally met expenses in the following decades, and payments on the life annuities sold during the 1420s, which often took up 70 percent of the city's ordinary receipt before 1450, gradually dwindled. The city also had to take constant measures to counteract the Bavarian counts' monetary mutations. The silver coin of Holland was devalued 42 percent between 1388 and 1433. The Leiden accounts regularly express sums in more stable foreign coins, generally Flemish, and then add their equivalent value in the current Dutch coin. The Burgundians stabilized the coinage until the 1460s, when the debasements of Charles the Bold caused Leiden to make a tentative effort to move toward a gold standard.

The nature of the expenses, receipts, bookkeeping, and fiscal bureaucracy of Leiden, together with the recurring theme of *ad hoc* measures to protect local finances from ruin at the hands of the territorial lord, thus show no striking departures from the patterns found for other cities in the Low Countries. This book is a solid and sober contribution to the history of urban finance that should be read by anyone who cherishes sanguine views of Burgundian administration.

DAVID NICHOLAS
University of Nebraska

DAVID NICHOLAS. *The Domestic Life of a Medieval City: Women, Children, and the Family in Fourteenth-Century Ghent*. Lincoln: University of Nebraska Press. 1985. Pp. x, 261. $26.50.

David Nicholas, whose previous work on political and economic history has demonstrated a solid grasp of Ghent's rich archival sources, ventures here

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into the new terrain of family history. His basic themes are enunciated in an introductory chapter and alluded to in succeeding chapters, which make up sections on the status of women ("Dragons of the Distaff"), the status of children ("Honor Thy Fathers"), and the clan family ("Pride and Lineage").

First, he argues that what Peter Laslett called the Western family (as distinct from the Mediterranean model), which scholars working in French history have traced no further back than the sixteenth century, can already be found reflected in the narrative and legal evidence for fourteenth-century Ghent. In support of this contention, he shows that the clan family generally refrained from interference in the affairs of the nuclear family, as long as father and mother were still alive and living together. Apart from aristocratic families, he finds little evidence of arranged marriages or of extended families living under the same roof. Moreover, the clan rarely exercised its right to confiscate the property of women carried off for marriage (even if abducted against their will), preferring to settle with the husband whom (in most cases) the woman preferred. Nicholas acknowledges, however, that his sources do not permit determination of ages at marriage: thus, one important element of the Western family model (comparable ages of spouses, suggesting companionate marriage) cannot be verified.

Second, he contends that women in late medieval Ghent were neither "systematically oppressed" nor "enveloped in an ether of Marian chastity" (p. 208). Nicholas's most interesting chapters on this theme deal with the economic status of women. Favored by Ghent's usage of the Parisian form of partible inheritance, with no special rights for males, women were often active as money-lenders and drapers and as members (usually, though not always, by right of a deceased spouse) of various craft guilds. They typically did not own land but nonetheless paid as much as one-fourth of the forced loans sometimes levied on wealthy burghers. Legal documents show widows generally conversant with details of their late husband's business, and, whatever their social station, "girls were not expected to sit around the house being decorative" (p. 96). Nicholas does not identify the authors whose views on the history of women he wishes to refute, which is unfortunate since this is a point where some comparative discussion would have been particularly helpful. One suspects that the involvement of women in the business affairs of Ghent was in some way related to the precocious economic development of Flanders.

Finally, Nicholas also maintains that evidence from Ghent contradicts Philippe Ariès's notions about the modernity of childhood. Nicholas's argument here is based on the fact that Ghent (like other towns in the Low Countries) required public declaration of the estate when one or both parents died while one or more of their children was still below the age of legal majority. But this arrangement can be interpreted as safeguarding clan rights (it was the kindred of the deceased parent to whom account was rendered), and, even if it is interpreted as safeguarding the child's rights, the arrangement does not address the subtleties of Ariès's argument.

In sum, in the context of an argument for the essential continuity of medieval and modern history, which not everyone will accept, Nicholas has presented a richly detailed account of domestic life in a major town, which all students of family history must henceforth take into account.

JAMES D. TRACY
University of Minnesota

GEORGES DUBY. *The Knight, the Lady, and the Priest: The Making of Modern Marriage in Medieval France.* Translated by BARBARA BRAY. Reprint. New York: Pantheon. 1985. Pp. xx, 311. $9.95.

This is the second and expanded version of Georges Duby's study of marriage among the high aristocracy of medieval France. In outline it is much the same as *Medieval Marriage: Two Models from Twelfth-Century France* (1978). Fleshed out with a larger and livelier cast of knights, ladies, and priests, the new version incorporates minor changes of emphasis, pares the endnotes, and issues forth in a glib translation.

It is a drama of power, sex, and, increasingly, money played out on the stage of northern French feudal society during the eleventh and twelfth centuries. The knight is the hero of the tale, both as callow, or seasoned, youth seeking adventure, glory, and female companions and as noble head of a male-dominated aristocratic lineage dutifully stewarding his family's honor and patrimony. The lady, whose body the knight pursues (if often for reasons of the body politic), assumes several roles in the piece. These include nubile sex partner, heiress, concubine, wife, discarded wife, adulteress, mother. All, however, are essentially bit parts, and, although the lady has a few more lines in this version, she still remains much in shadow. Her significance as a person appears to be dwindling in this emphatically masculine society of lords, vassals, and virile heroes, and she is handed from bed to bed with seemingly few protests, to judge by the sources and Duby's skillful exposition of them. Certainly they give her far less to say than the leading man who, we are told, both disdains and fears her in any guise. The knight shares this unfavorable opinion of womankind with the priest, the main supporting actor, with whom the knight often clashes when he changes partners. The priest, especially one of the Gregorian reform persuasion, functions essentially as a chorus here,

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deploring and denouncing the knight's penchant for connubial excess, incest, bigamy, and other mating habits that disturb peace and order in the world, not to speak of their flagrant disregard of the church's morality and, above all, authority.

The book explains how and why lengthy discussion and open confrontation between knight and priest produced, by the end of the twelfth century, accommodation of their divergent views on the theory and practice of marriage. If the knight eventually conceded that the priest ought to have more to say about his matrimonial commitments, newly sacramental and indissoluble marriage was a small price for the knight to pay for continuing to manage his own and, often, others' matrimonial alliances much as he pleased. Indeed, his is a success story. The priest might regulate many of the terms of the marriage bond, but the knight maintained his control of it as an indispensable means of acquiring and keeping friends, prestige, and wealth, objectives that dictated the giving, taking, or withholding of brides in accordance with his dynastic responsibilities, ambitions, and opportunities.

This is a key study in Duby's work on a mighty and highly self-conscious and self-important social group, particularly its leading men. Matrimonial concerns differed somewhat in other regions and on less-exalted social planes, and this very readable yet complex and thoughtful book offers many suggestions for comparison and debate about profound changes occurring at a different pace in other places and at lower altitudes.

HEATH DILLARD
New York, New York

ANN K. WARREN. *Anchorites and Their Patrons in Medieval England*. Berkeley and Los Angeles: University of California Press. 1985. Pp. xiv, 375. $37.50.

The subject of hermits and anchorites in medieval England, last dealt with on a broad scale by Rotha M. Clay in 1914, has long cried out for an authoritative, modern treatment. Ann K. Warren's preface, while paying due tribute to Clay, invites the comparison, so it must be said at once that the work at hand, though in its way sound, is in that light seriously hampered on two counts.

The first is that, whereas Clay dealt with her subject by topics all related in one way or another to the hermits themselves, Warren's real interest seems rather to lie in their patrons. An inquiry into the social and economic nature of the support that the anchorites received from royalty, the aristocracy or gentry, merchants, and clerics lies at the heart of her book. The analysis of this support is undeniably thorough, as shown by the twenty-two tables that lay out the author's findings on transference of alms and other quantifiable matters. But data of this sort become the tail that wags the dog; at the least, the book is misleadingly titled.

The other major problem lies with the author's decision to start the story only in the twelfth century, on the grounds that "the earliest clear picture of English anchoritism emerges" from documents of that era (p. 17). (Warren's distinction between hermits as free to move about and anchorites as having taken vows of permanent stability may have been methodologically necessary but is in reality artificial.) The roots of English anchoritism lie squarely with Cuthbert and Guthlac, and to ignore those roots is to truncate the story, which weakens the first part of the book on the anchorites themselves.

These large flaws aside, some potentially fascinating themes have been passed by or left unexplored. One example is the odd phenomenon of anchoresses near castles. Cells are mentioned in passing at Dover (where a succession of recluses lived), Pontefract, Dublin, Bristol, and the Tower of London, but no overall explanation is suggested. Another such theme is the question of the cultural level of the recluses. If the so-called St. Albans psalter now at Hildesheim was, as is generally thought, really made for Christina of Markyate—a fact nowhere mentioned here—we have an early twelfth-century anchoress immured with a highly sophisticated and sumptuous Latin devotional document; how does this square with the traditional understanding of the austerity of the enclosed life?

For what it actually does, rather than for what it purports to do, Warren's book is thorough and useful. But the subject is far from exhausted. Any further workers in the field will, however, be grateful to her for the mass of material she has accumulated. Especially praiseworthy are a full bibliography and an appendix succinctly detailing thirteen English anchorite rules.

RICHARD W. PFAFF
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ALDO LANDI. *Il papa deposto (Pisa 1409): L'idea conciliare nel Grande Scisma*. (Studi Storici.) Turin: Claudiana. 1985. Pp. 332.

The title of this excellent book is a little misleading. Aldo Landi's work is not just a history of the Council of Pisa—although the council and the preceding events are central concerns—nor is it just about conciliar ideology. Rather, he has skillfully blended political, ecclesiastical, and intellectual history to produce a stimulating and rewarding sketch of the Great Schism, the development of conciliar thought in response to it, and the path that led the Council of Pisa to assert its right to depose a pope. Pisa was a failure in many respects. It did not end the schism,

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and, by electing a new pope without effectively deposing the old, the representatives at Pisa presented Europe with the spectacle of three, rather than two, popes. Nevertheless, Landi argues that Pisa's triumph was to unite secular and ecclesiastical leaders behind the *via concilii*. After Pisa, Constance was a foregone conclusion.

Landi has divided the book into four parts: the period from 1378 to 1408, the events immediately leading to the Council of Pisa, the council itself, and a short epilogue in which he follows the course of conciliarism to the Council of Pisa of 1511–12. Each part is cut up into a large number of one- to two-page sections. These small divisions do not hinder the pace of the narrative but permit Landi gracefully and easily to change his focus from the political to the intellectual and to enlarge the scope of his vision from the courts at Rome or Avignon to the diplomatic problems caused by the Hundred Years' War.

In the first part Landi analyzes the elections of Popes Urban VI and Clement VII in 1378 and the subsequent political and intellectual developments to the beginning of the fourteenth century. Neither pope, in his view, can be called an antipope. Both the Roman and the Avignonese popes had powerful arguments they could employ to support the legitimate claims of their line. Intellectuals recognized the impossibility of deciding between the two, and by 1398 the *via cessionis* (both popes renounce their office simultaneously) had become a commonly accepted method of resolving the crisis. Yet what was theoretically feasible was soon seen as practically impossible. The popes remained obdurately opposed to taking meaningful steps to reunite Christendom. They would not act in concert, and efforts to withdraw obedience from the rival claimants collapsed because of intractable rivalries among secular rulers.

A council became the only alternative. French and Roman cardinals abandoned their popes in 1408 and called for a council to end the schism. They met in Leghorn and planned a council for the following year. Eventually, Pisa was chosen as the place. Landi's account is particularly valuable for its lucid explanation of the intricate political negotiations between France, the Italian city-states, the kingdom of Naples, and the Holy Roman Empire. In March 1409 the Council of Pisa was convened. By June it had deposed both popes and immediately afterwards a united conclave of twenty-four cardinals elected Alexander V. Pisa's importance, Landi points out, was that a council had asserted its right to depose a pope on the basis of the canonistic and theological doctrines that had evolved in theoretical discussions during the last two centuries.

Landi's work does not, for the most part, break new ground, but he has mastered the secondary literature in English, French, and German. The detailed bibliography is a good guide to the current literature. The book is also a first-rate introduction to the sources and a balanced survey of current historical opinion. In a field where differences of opinion have been quite sharp, he has avoided polemics scrupulously. Last, the book is clearly and engagingly written. It is a pleasure to read.

KENNETH PENNINGTON
Syracuse University

JOSÉ MARÍA MONSALVO ANTÓN. *Teoría y evolución de un conflicto social: El antisemitismo en la Corona de Castilla en la Baja Edad Media*. (Historia de los movimientos sociales.) Madrid: Siglo Veintiuno. 1985. Pp. xii, 351.

The historiography of the negative aspects of the Jewish and *converso* experience in Spain has reached the proportions of a minor industry of which this book is yet another product. The volume is true to its title. With rare exceptions necessary to the unfolding of the account, such as the exploits of Vincent Ferrer, José María Monsalvo Antón does confine himself to Castile. The book does describe an "evolution," insofar as the second part surveys the events in periodic sequence from the thirteenth century through the expulsion. It is the word "theory," however, that best characterizes the nature of the monograph. On the basis of the spate of literature on this subject produced in recent decades, much of it reflecting archival research, there has grown a tertiary literature that attempts a sweeping vision and theoretical analysis on a grand scale. Monsalvo Antón's work continues in this direction and attempts to provide a broad perspective on the basis of evaluations of the secondary and tertiary works without himself providing original research. This is perhaps fitting for the series in which it is published, but the reader should not expect to find any new and original contribution, which the wealth of archival material in Spain might yet produce. He or she will find proper regard paid to *histoire des mentalités* and to the question of whether the Jews are a class, caste, or religion as well as a lengthy chapter on a "theoretical-practical model of anti-Judaism—discrimination, segregation, inferiority" (pp. 135–96).

Perhaps the work's significant contribution is its perspective. Castilian anti-Semitism is discussed not as a Jewish problem but in terms of its meaning for Spanish history. Surveying the scholarship of such recent writers as Henry Kamen, Maurice Kriegel, Pilar León Tello, Angus Mackay, Luis Suárez, J. Valdeón, and others as well as the older scholarship (of José Amador de los Ríos, Yitzhak Baer, Luís Sánchez Albornoz, and so on), the author tries to

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come to grips with the basic issue of how Castilian society construed the Jewish element in its population. Inevitably he is forced into a synthetic approach. To give one example, although he rejects the concept of the Jews as "caste" in economic terms, he later accepts it because of their "untouchability" from the point of view of religion and ideological background.

Despite contradictions and inconsistencies, the book renders a service by putting the exaggerated role of the economic impact of Castilian Jewry into perspective and stressing the diversification of Jewish professional and trade activities. In this regard, some comparative approach, such as with Portugal, would have been illuminating, and the misleading comparison with Spanish Muslims might better have been omitted (p. 102).

FRANK TALMAGE
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MODERN EUROPE

HUGH TREVOR-ROPER. *Renaissance Essays*. Chicago: University of Chicago Press. 1985. Pp. viii, 312. $22.50.

The reviewer who recently suggested in this journal (*AHR*, 91: 381) that few historians "would assert now that our knowledge justifies confident generalizations" does not express the opinion of Hugh Trevor-Roper. Although many of these essays are about limited topics, Trevor-Roper's penchant for alluring generalizations is fully evident. Not all will agree with these generalizations. Yet no reader can fail to be challenged and stimulated, all the more because these essays are written with the clarity and elegance that have made their author one of the most readable of contemporary historians.

Although most historians associate Trevor-Roper's name either with the controversies over the English gentry and the origins of the Civil War or with the debates over Hitler, he has long been active as an intellectual and cultural historian of the Renaissance period. This aspect of his work is reflected in this collection of thirteen essays, three of them previously unpublished. Most are occasional pieces, either conference addresses ("John Stow," "Richard Hooker and the Church of England," "Queen Elizabeth's First Historian: William Camden," and "The Culture of the Baroque Courts") or review essays ("Erasmus and the Crisis of Christian Humanism" and "The Lisle Letters") or commemorative volumes ("Sir Thomas More and *Utopia*" and "The Sieur de la Rivière"). The rest are *haute vulgarisation* ("The Doge Francesco Foscari," "The Emperor Maximilian I as Patron of the Arts," and "The Outbreak of the Thirty Years' War"). None is comparable in importance to "The Social Causes of the Great Rebellion" or "The General Crisis of the Seventeenth Century," yet each has something of value to say.

It would be wrong, however, to look at this volume only as a collection of contributions to thirteen different topics. Unlike the reviewer quoted above, Trevor-Roper does believe in generalizations, and here he puts forth certain broad interpretations that call for comment. Several of these essays deal with the influence of Platonism on Renaissance culture. He sharply distinguishes the Florentine Platonists and their northern disciples, with their tendency toward magic and mystification, from the reformist Platonism of John Colet and More, in which he sees the rigid, totalitarian tendency of Platonist thought becoming dominant. Perhaps more questionable than this interpretation of More's *Utopia* is the association of Erasmus with this tradition. Although Erasmus certainly shared some of Colet's and More's enthusiasm for Plato, neither his political thought nor his conception of religious reform has the ring of Platonic authoritarianism. By making Erasmus into a Platonist, Trevor-Roper weakens his own case for treating him as a Christian humanist. Precisely because he is right in emphasizing the antihistorical nature of Platonism, he is wrong in making a Platonist out of Erasmus, one of the pioneers in the transformation of the humanists' philological method into an instrument of historical criticism. In fact, whereas the essay on More treats Erasmus as a Platonist, that on Erasmus gives only occasional attention to this strange opinion. A quite different approach to Platonic influence appears in "The Paracelsian Movement." Trevor-Roper investigates the connections among the magical-mystical brand of Platonism, the effort to introduce chemical remedies into medicine, and the affinities of Paracelsian thought for Protestantism.

These essays also present other broad generalizations: for example, the author's insistence that "court" and "country" are the most significant polarity in European politics and society of this age and his contention that the splendid courts of the baroque age masked a dangerously unstable social, economic, and political reality. Trevor-Roper repeatedly tries to demonstrate that the English Reformation was marked by a unique emphasis on historical continuity and Erasmian moderation, but thereby he underestimates (even while occasionally admitting in words) the attraction that Protestant theology, especially Calvinism, had for sixteenth-century Anglican thinkers. Thus, he makes the line between Anglican and Puritan far more distinct than it really was. Nevertheless, even when his reader does not agree with him, Trevor-Roper's essays are challenging and stimulating. What a rich

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storehouse of ringing generalizations to quote while setting examinations for graduate students! How could we ever do without them?

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GEOFFREY PARKER. *The Thirty Years' War*. Boston: Routledge and Kegan Paul. 1984. Pp. xliii, 340. $39.95.

The corridors of overflowing stacks devoted to the Thirty Years' War in most major libraries testify to the continuing fascination of that grisly conflict. And the pace of publication has not slackened in recent years. Indeed, the majority of the works cited in Geoffrey Parker's twenty-two-page bibliographic essay were written in the last three decades. His purpose is to summarize that work for a more general audience, to organize it into a coherent survey, and to provide an updated diplomatic history of the war for those still relying on C. V. Wedgwood and Georges Pagès.

Cramming the endless intrigues into little more than two hundred pages while restoring to proper significance the events of Northern and Eastern Europe and the activities of a dozen lesser German princes is no mean achievement. Parker not only outlines the main events, policies, and protagonists of the war but also adds a brief discussion of soldiering, of the social and economic effects of the fighting, and of the political aftermath. To cover some areas that not even his voracious reading could encompass, he has given about one-third of his text to nine colleagues, who write on unfamiliar subjects such as Danish policy. The result is a whirlwind survey, presumably aimed either at students or at nonspecialists who need a modern introduction to one of the most confusing and incomprehensible episodes in European history. Accompanied by excellent indexes, informative maps, and well-chosen illustrations, the book should serve these purposes for some years to come.

Yet it will not be easy going for those without experience in seventeenth-century studies. Some of the tables (for example, those on pages 155 and 202) complicate rather than clarify; far too many names pop in and out; identification is often haphazard (we are told who the duke of Buckingham was, but not until their second mention who the Bohemian Brethren were); and students may be forgiven for asking why Lower and Upper Austria, but not Inner Austria, are in the maps. In general, there are too many actors and dates and not enough concern for the shape of the drama. Do we need the names of bit players such as Alexander Erskine and Marguerite of Lorraine in so short an account? Or the dates of six treaties in a single sentence? They serve only as a stimulus for identification quizzes.

The impression of extensive research notes compressed on the printed page is reinforced by the marks of hurried writing (the word "only" is regularly misplaced, and Philip IV has "her" allies). For the seventeenth-century historian unable to keep up with a large and complicated literature, such shortcomings do not detract from the enormous service Parker has rendered. For students, however, they may revive the traditional problems of this subject: What is worth remembering? How does one distinguish between matters of import and mere details? The account also has imbalances of which the inexpert reader should be aware.

Although not billed as such, the book is essentially a history of the war's diplomatic maneuverings. Military tactics receive some attention, but no battle set pieces or explanations of particular victories appear. And, though economic developments, domestic politics, and intellectual and social forces are touched on, they do not receive the prominence they deserve in a comprehensive narrative. In particular, we gain little understanding of the different regions of the empire, especially their varying social, economic, and political structures. Without that rootedness, diplomacy sometimes receives more credit than is warranted (for Brandenburg's success at Westphalia, for example, or for Saxony's acceptance of the Peace of Prague), and some judgments seem less than secure. Was Stadtlohn really a more decisive Catholic victory than Nördlingen? Can Catholic princes who supported the Edict of Restitution be regarded as committed to the rights of German princes? And the information about the relationship of disease to overcrowding and malnutrition is out of date.

Nevertheless, although regretting that Parker did not create the definitive introduction to the subject that this book could have been, one must still applaud his achievement: the most comprehensive current account of the complex international issues of the Thirty Years' War.

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JOHN BOSSY. *Christianity in the West, 1400–1700*. (OPUS.) New York: Oxford University Press. 1985. Pp. xii, 189. Cloth $18.95, paper $6.95.

In a tightly packed, highly personal history John Bossy confronts the Reformation, Protestant and Catholic, from the standpoint of a supple and vigorous medieval past. Bossy sees the traditions—in the singular—of medieval religion as a working relationship among ordinary believers, their priests, canon lawyers, and theologians. Lay people and

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clergy maintained a healthy balance as together they came to define and experience the sacraments in ways reflecting both secular concerns and aspirations toward the sacred. Bossy's secular concerns are not, however, famines and plagues, for his Christianity stands in a different plane from demographic crisis. Nor do the troubles of the institutional church matter very much.

By the fifteenth century, Bossy argues, the search for fellowship had become the chief goal of Christian living. Envy, pride, and anger were the worst of the seven sins because they drove people apart. Christians sought fraternity through religion, since in everyday life they seethed with mutual hostility. As an explanation, Bossy invokes the image of limited good, which postulates that in a peasant society one person's gain is necessarily another's loss. But George Foster derived that image from a Mexican Indian village that had raised a bunker against the outside world (*Tzintzuntzan* [1967]). Bossy's peasants and townspeople are much more varied and dynamic.

The unity that Christians sought turned them against those outside their communion. Bossy regards the increasing constraints on Jews in Central Europe and the vise that gripped Hispanic Jews and converts as an inevitable, though unpleasant, result of heightened Christian consciousness. He recognizes but discounts the force of one of the best of medieval traditions: cohabitation among the religions of Iberia.

Such reservations aside, Bossy's survey of late medieval religion is magisterial. He discusses theology and church law with consummate ease, has an acute appreciation of the liturgy, and convincingly depicts people freely practicing a faith they understood. His interpretation of changes slowly underway in the fifteenth century, including a new privacy and an incipient shift from the seven sins to the Ten Commandments, is particularly impressive.

These and other changes were intensified by the Reformation, which exploded the old understandings about solidarity. Over time, focus on the word gave a hard, literal edge to bibles and liturgies. Calvinists, striving for a palpable sense of the holy, reshaped the Sabbath into a rite of exclusion. Meanwhile, the examination of conscience came to distinguish devout and well-bred Catholics from the rest. The Catholic majority, Bossy speculates, may have become more prone to superstition, which thus becomes a consequence of reformation. In general, religion was reduced to what could be taught and what could be inculcated through discipline. The villains of the piece are not the reformers but the humanists, whose ethics of civility were the bedrock of the new segregations.

Although emphatic in his preferences, Bossy is experimental in his methods. He seeks, in a sinuous, self-questioning fashion, to articulate the quiet shifts in attitudes and assumptions that underlie later, apparently sweeping developments. At all times, changes in the mind and spirit condition those in the material world. And irony is one of Bossy's chief modes of reflection. The Catholic church, for example, having lost to secular authority its role as a guarantor of social peace, filled the gap by slowly taking a much clearer and more forceful stand than previously on sex. Bossy's discussion of causation is as challenging as is his dissection of the Reformation.

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BERNARD M. G. REARDON. *Religion in the Age of Romanticism: Studies in Early Nineteenth-Century Thought.* New York: Cambridge University Press. 1985. Pp. ix, 303. $39.50.

The purpose of Bernard M. G. Reardon's book is stated quite simply: "to examine certain aspects of the Romantic movement of the early nineteenth century as it expressed itself in the philosophical and religious thought of the period" (p. vii). Fundamentally opposed to the rationalist syndrome of the eighteenth-century Enlightenment, the Romantics were never intentionally obscurantists, and, as Reardon rightly points out, they were not opposed to the use of reason. They only opposed the narrow, and somewhat naive, usage of it as found among the philosophes, with their Newtonian perspective and Holbachian application. Rather, for the Romantics, reason should be applied to all aspects of life, not the least of which was religion. The Romantics were therefore concerned with Christianity, and Reardon offers scholarly and informative chapters on the ideas of such noted writers of the era as Friedrich Schleiermacher, G. W. F. Hegel, K. F. A. Schelling, Ernest Renan, and Auguste Comte.

Generally, these writers held certain ideas in common, however amorphous this commonality may appear. Referring to the ideas of the Romantic era as a "treasure-house in disorder" (p. 2), Reardon believes that "the essence of romanticism . . . lies in the inexpugnable feeling that the finite is not self-explanatory and self-justifying, but that behind it and within it . . . there is always an infinite 'beyond'" (p. 3). Therefore, the coincidence of what are presumed to be opposites, namely, the finite and the infinite, permeated the thought of the Romantics and can be witnessed, for example, in Schleiermacher's concept of *Gefühl*, Hegel's of *Geist*, and Félicité Lamennais's of ultramontanism. This desire to find the infinite within the finite expressed itself in an intense emotionalism, in egotism, in a nascent historicism, and in aestheticism. Reardon correctly in-

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fers that the Romantic era is as important for our age as were the political events in France of 1789.

The correctness of Reardon's views notwithstanding, the structure of the book lacks cohesiveness. After an introductory chapter explaining the relationship of romanticism to idealism, there follow eight chapters in which individual thinkers are considered in succession. The final chapter of the book deals with one thinker and is entitled "Ernest Renan and the Religion of Science." Thus, the book ends without a summary or conclusion. Since the author clearly has a firm grasp of the field of romanticism, some critical conclusion would be appreciated. Nevertheless, Reardon has written an informative work on a period of history that has too often been misinterpreted as "reactionary" and that has too frequently been dismissed as an era of irrational religious sentimentality. His book aids in correcting such erroneous impressions.

HARRY J. AUSMUS
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STEPHEN MENNELL. *All Manners of Food: Eating and Taste in England and France from the Middle Ages to the Present*. New York: Basil Blackwell. 1985. Pp. xii, 380. $24.95.

Stephen Mennell has undertaken a sociological analysis of one aspect of "the civilizing process," seven centuries of change in eating and attitudes toward food in France and England. His aim is to describe patterns of interrelationship between the two countries and between classes and subgroups within them. He uses cookbooks, pamphlets, as well as food trade and domestic periodicals and draws on the work of social historians, especially Norbert Elias. Historical chapters are interspersed with excursuses on appetite, gastronomy, and dislikes of certain foods.

Mennell finds that medieval cuisine was an international court style practiced in England and France. Differences were greater between the strata of society in a single country than between corresponding strata in the two countries. Between about 1500 and 1789 national styles became more pronounced. Change was accelerated by the coming of printed cookbooks, which transmitted information to a wider audience. In France *la grande cuisine* developed out of the "courtly" style. Mennell argues that, as the aristocracy lost its traditional functions, French court life became centered on competitive participation in highly nuanced court ritual. The *noblesse campagnard*, cut off from the center of power, could not function as originators of style. The importance of food as a cultural expression can be traced from the mid-seventeenth century. Prefaces of cookbooks show a consciousness of change, a belief in progress, a desire for order and correctness, and, by the mid-eighteenth-century nouvelle cuisine movement, an emphasis on elegance.

In contrast, the English upper class, less dependent on royal favor than French courtiers, was freer to pursue more practical activities. They lived much on their country estates, and their style of eating was more like that of the strata beneath them than was the case in France. English cookbooks, often written by women, abounded. They divide stylistically into "courtly" cuisine (which died out after 1730) and "country" cuisine; tavern cooking appeared later in the eighteenth century. In France the development of restaurants after the revolution brought *haute cuisine* into the public sector, and the métier of chef was gradually professionalized; women cooks were largely restricted to working in the smaller restaurants and private homes. In England women did somewhat better, but working cooks never achieved the professional prestige of their French counterparts. English cooking was "decapitated" (p. 204) when, in the nineteenth century, the upper class turned to France for culinary models, thus tainting indigenous cuisine with lower-class associations. Mennell has made an interesting analysis of nineteenth- and twentieth-century food trade and women's magazines and found that the French trade journals have always emphasized food preparations and women's magazines the pleasure given by it, whereas English trade and domestic magazines have alike emphasized economy. Today English commercial cooks are being "deskilled" by technological change (p. 199). Class differences are diminished, and more styles of cooking are available to all.

The author surveys a vast field; more research will be needed to test his theories. His survey of France focuses largely on the extremes of wealth and poverty and not enough on the middle classes, where much of the regional and bourgeois cuisines have been practiced. Why, since the English upper class lived more in the provinces than did the French, did it persist in affecting a French culinary style? The underlying question in this work seems to be, "why is English cooking worse than French?" The cooking in many countries is. We might rather ask why fine cooking has been so important to French identity for so long.

BARBARA KETCHAM WHEATON
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JACK SNYDER. *The Ideology of the Offensive: Military Decision Making and the Disasters of 1914*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1984. Pp. 267. $32.50.

The monumental error of the general staffs before World War I in overestimating the efficacy of offen-

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sive strategies and underestimating the advantages afforded by changed weaponry to defensive strategies has long been recognized in historical literature. Explanations abound depending on the predilections and expertise of the historian, but few scholars are equipped to rise above a particular national setting or type of history. It is to Jack Snyder's credit that he attempts to break through these barriers with a cross-cultural, interdisciplinary approach and selects France, Germany, and Russia for examination. This enables him to examine empirically the validity or universal applicability of specific hypotheses. The approach promises and delivers a number of fresh new insights as well as confirmation of old ones. For example, the preemptive mode of thought—that, given the probability of major conflict, one must strike the first decisive blow and impose the terms of the conflict on the enemy—finds more than adequate confirmation, as does the propensity to seize "windows of opportunity" (to force the contest before the enemy has the opportunity to remedy his deficiencies) and to perceive "the necessary as possible" (to assume the feasibility of a vital strategic goal, for example, the conquest of eastern Prussia). He profusely illustrates the perceptual distortions arising out of the effort to impose rationality on a medium of incalculables; that is, out of the multiplicity of possible scenarios, military establishments select out those that reinforce their self-image, institutional interests, and the doctrinal simplicity of field regulations. The author makes a compelling case that these factors conspire greatly to increase the likelihood of war and its protraction. The author clearly finds lessons that apply to the nuclear age as well (the modern military professional is "over-socialized" to the realities of international anarchy and the interests of one's own nation-state).

Snyder nevertheless rejects transnational cultural explanations of offensive planning and insists that the reasons are purely military and peculiar to each country. He insulates himself from addressing broader possible influences by claiming that war plans are made by professional soldiers, not civilians; indeed, the examination of war plans, and those few who constructed them, become his exclusive concern. The result is to undercut any meaningful general conclusions—other than the inherently flawed character of offensive strategy, about which he overgeneralizes—that a comparative approach might suggest. Thus, the work contributes far less than one is led to expect to an understanding of the forces that impelled Europe toward universal war. Even more distressing is the pretense of methodological exactitude by employing motivational analysis and other tools of social science, worked into graphs and charts that purport to represent the exact locus of each national strategy. The author appears to commit the same error as his military thinkers; namely, he screens motivational bias behind the pseudorationality of a scientific doctrine. The result is an artificially deductive method of presentation resembling Aquinas's *Summa*. Still, the material, particularly that on France, is quite rich and demonstrates that a straight line from du Picq to Foch did not carry over Catholic, antirepublican values into strategic thinking. On Germany Snyder includes little material that is new, but the chapters on Russia are quite welcome in view of the dearth of secondary works (even in Russian). His use of the generally inaccessible work of A. M. Zaionchkovskii and several Soviet authorities puts new information in circulation on the "defensive" strategy of General Iurii Danilov and the "offensive" strategy of M. V. Alekseev, but the antecedents that might be found in the labors of Generals M. I. Dragomirov, G. A. Leer, and A. N. Kuropatkin remain unexplored. Altogether, this is a much-needed and useful book, but it represents a foundation on which to build rather than a completed edifice. A broader cultural and social approach might still represent a useful extension of the comparative perspective.

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THOMAS G. AUGUST. *The Selling of the Empire: British and French Imperialist Propaganda, 1890–1940*. (Contributions in Comparative Colonial Studies, number 19.) Westport, Conn.: Greenwood. 1985. Pp. xii, 233. $37.50.

Thomas G. August introduces a new consideration in the economics of empire: the company sales pitch. Alongside the rhetoricians urging imperialism on were the institutions supported by governments in an effort to convince the folks at home that having the flag fly abroad was a good thing, worth supporting. "Buy British," as the interwar alliteration admonished. Yet little was directly bought, either goods or propaganda, by the public at large. In the interwar era, with which this book is primarily concerned, public attention was focused on more pressing local needs, on nearby distractions. At the Wembley Empire Exhibition of 1924 the amusement section was enjoyed most.

The book has a somewhat misleading title, suggesting what is not emphasized. A more descriptive title might have been "The Salesrooms of Empire." August concentrates not on the slogans but on the organizations in which they were worked up and fitted out. The discussion of the exhibition galleries of the Imperial Institute in London includes this comment: "Poor lighting, heating, and appearance were constant sources of criticism" (p. 72). What these inadequate conditions surrounded, we never

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see; what was displayed on the walls is barely mentioned. Similarly, the discussion of a British campaign of 1926 states that "posters were displayed on public hoardings in 450 cities and towns" (p. 76). What was displayed on the posters is not described. Then again, a series of lectures produced by the British Broadcasting Corporation and undertaken in conjunction with the Colonial Office is explained in terms of format and editing, but the contents of the lectures are only sketchily given. To state that certain "sensitive subjects . . . received generous blue-penciling" (p. 98) is interesting; better would be examples of what was blue-penciled and what was not. Jean Renoir's famous, if personally embarrassing, film, *Le Bled*, done for the centennial celebration of colonial Algeria, is dismissed in one line (p. 103). As a major governmental excursion into filmmaking and one of the most elaborate on-site films of the time, *Le Bled* deserves more attention than this.

The author contends of the purpose of the study that the "focus . . . is the projection of the overseas empire in the mother country" (pp. 61–62). The focus—the manipulation and direction of the apparatus—is provided. We see what the projector is doing; we do not see the projection. To evaluate the effectiveness of the effort, the reader must know what was distinctive in the materials presented: what was emphasized, "packaged" from that bargain basement stock of ideas that stood as the justifying ideology of empire. Moreover, who if anyone, consumed the materials? What was the public reaction?

Granted, much of such illustrative material, from the British perspective, is already found in John M. MacKenzie's *Propaganda and Empire: The Manipulation of British Public Opinion 1880–1960* (1984). Even so, nothing comparable is available for the French, nor is an evaluation of the distinctly governmental effort, as against the broader public one, possible without such evidence provided to the reader.

This is a useful book that might have been commendable had the "goods" as well as the showcases been displayed—had "consumer" reaction been assessed.

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JOHN W. YOUNG. *Britain, France, and the Unity of Europe, 1945–1951*. Leicester, England: Leicester University Press; distributed by Merrimack Publishers' Circle, Salem, N.H. 1984. Pp. xi, 228. $24.00.

John W. Young is one of the foremost of a group of young English scholars who, following the lead of Alan Milward, have made the history of European international relations in the aftermath of the Second World War a new and exciting field. Inevitably much of their work was originally based on the records of the British Foreign Office preserved at the Public Records Office. Of all the European records these have been the most open and consistently reliable to all scholars, irrespective of their seniority. And it is only fair to Young to comment that his book appeared simultaneously with the release of the important military records of the Brussels Treaty Organization, which show, *inter alia*, the extraordinary scale of British military aid to France and to Western Europe between 1945 and 1948 (some 1 billion dollars at pre-1949 sterling devaluation exchange rates). Also, his account of the British and French roles in the establishment of the North Atlantic Treaty Organization (NATO) represents only an early step in the correlation of national versions of the origins of the treaty that is still in progress.

Given these limitations, however, Young's book is an excellent illustration of the merits of the British school of international history. Young acknowledges the aid and advice of some of the leading British practitioners. But guidance and advice alone do not account for the depth of Young's approach or the remarkable succinctness of his style.

The main burden of Young's work is the exposition of the vision of a European bloc arrived at simultaneously by the senior officials of the British Foreign Office and the new Labour government's foreign secretary, Ernest Bevin, in the summer of 1945; how this developed through the strains put on Anglo-French relations by the crisis of 1944–45 in the Levant and by French policy toward Germany; and how gradually—through a series of weak, divided, and uncertain French governments, viable only by virtue of the absence of a threat from the Left until the Communist party moved into opposition in 1947—Britain moved into an alliance with France just in time to enable the two countries to unite on a positive reply to the Marshall proposals of June 1947 and to go on to the negotiation of Western union and NATO.

In that moment of success were, however, the beginnings of the subsequent parting of the ways. Britain, obsessed by defense and the need to reanchor America to the security of Europe, stayed with NATO and the Organization for European Economic Cooperation. France, beset both by nightmares of a surprise Soviet strike and the renaissance of a German state, moved toward anchoring Germany to the West by the Schumann Plan and the European Defense Plan. By 1951, the end of Young's tale, France and Britain were at odds again. To cover all this in approximately one hundred eighty pages inevitably involves merely hinting at much of the significant detail. But as an introduction to the study of these vital years in the postwar

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history of Europe, Young's book is to be highly commended.

D. CAMERON WATT
London School of Economics

ROLF STEININGER. *Eine vertane Chance: Die Stalin-Note vom 10. März 1952 und die Wiedervereinigung; Eine Studie auf der Grundlage unveröffentlichter britischer und amerikanischer Akten*. Bonn: J. H. W. Dietz. 1985. Pp. 158.

In 1952, during the final stages of negotiations between the newly established West German government and the Western Allies over the integration of the Federal Republic into a West European defense system and an end to the occupation, the Soviet government handed a note to the ambassadors of the United States, Britain, and France. It called for a unified Germany that would be unoccupied, could maintain its own armed forces and armaments industry, would be subject to no restrictions on its economy, would have freedom for democratic organizations and parties, would restore full citizenship to former soldiers and Nazis except convicted war criminals, and would be barred from joining any coalition or alliance against any of its World War II enemies. West German chancellor Konrad Adenauer and the Western Allies moved forward with their negotiations, however, which culminated in the end of the occupation and West Germany's joining the Western alliance. Some have argued that here the last chance for German reunification was lost or passed over deliberately.

This is the central message of Rolf Steininger's book, which is the text of his introduction to a volume of documents primarily from United States and British archives. The title for the documentary publication—*Eine Chance zur Wiedervereinigung?*—still contains a question mark: in Steininger's analytical introduction that question mark has disappeared. Steininger is certain that Adenauer deliberately ignored the opportunity and, in fact, took a harder line than the Western Allies. Against those authors offering different interpretations, he argues this position in detail on the basis of his reading of Adenauer's views, the relevant British and American archives, and such French and other material as he was able to use.

Parts of Steininger's view that Stalin was indeed serious merit careful thought, especially his point that the reversal of the French Communist party's position on allowing Germany to rearm reflects new orders from Moscow. But he has not thought through the implications of this for his own assessment of Stalin's promise of free elections. The Western Allies were promised such free elections in Poland also; they are still waiting. In 1952 a war was going on, a point that Steininger notes only in passing but that was much on the minds of those involved in the war or concerned about the possibility of one in Europe. Steininger waited with his publication until the British archives were opened; without access to the Soviet ones, it would have been wiser to keep the question mark.

GERHARD L. WEINBERG
University of North Carolina

HAIG SIMONIAN. *The Privileged Partnership: Franco-German Relations in the European Community, 1969–1984*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 407. $34.50.

F. Roy Willis produced a perceptive account and analysis of France and Germany in the first two postwar decades and their collective roles in the emergence of the "New Europe." Haig Simonian, a journalist for *The Economist*, has attempted to chronicle the more uncertain and troubled recent years. His achievements are much more limited, and contemporary historians will glean less from his assessment, even though it is well informed and well argued.

Two major lines of thought dominate this Paris-Bonn entente for Simonian: the major roles of individual statesmen and the key issues of enlargement and the agricultural crisis and moves toward monetary integration. The author is more successful when he views this critical relationship through the sagacious and accommodative diplomacy of Brandt and Pompidou, Schmidt and Giscard d'Estaing, and Kohl and Mitterand. Given the more difficult problems of the Schmidt-Giscard era (1972–80), including the oil price explosions, inflation, stagflation, and rising protectionist tendencies, Simonian focuses on the leaders' modes of cooperation in leading the European community. If the emphasis is on the practice of statecraft and the usefulness of a regional economic organization as a means of achieving major political priorities through a convergence of Franco-German interests, this work is far more than adequate.

In its selection of three cardinal controversies and their domination of those years in the two national capitals and at Brussels, the author has partially missed the complexity and extensive interplay of forces. In searching for those factors that caused the frequent diminution of national ambitions and the development of interdependent ties, we are given too much of a reductionist case. Without denying the significance of the Common Agricultural Policy, the European Monetary System, and the British entry into the European community as vital points of conflict or consensus, I wonder if equally important variables such as alliance politics, Third World

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relations, and particularly Middle East politics have not been downplayed. Simonian does not analyze in enough detail the priorities of NATO and the goals of the European community, the drive to redefine and restructure ties between members of the European community and their former colonies (which resulted in Lomé I and II in 1975 and 1979), and the interjection of transatlantic divergencies based on energy and investment. The turbulent era of detente and the erosion of detente necessitate a broader and deeper probing to explain the part European integration plays in Franco-German reconciliation and vice versa.

If what Paul Taylor has characterized as the gradualist process of integration and intergovernmentalism of the European community is the critical element of this period, then institutional aspects, especially those concerning structural change and continuity, also need to be more fully explored. Given the near absence of a wide range of interrelated issues adjacent to the Common Market and the minor role attributed to organizational developments, the book defines a rather narrow and therefore limited spectrum of historical interests. If the growth of the habits of collaboration, consultation, and convergence is central to this tale, then those elements that have fostered or deterred more French-German cooperation must be examined.

On personalities, monetary issues, farms, and enlargement tests, Simonian has provided interesting insights into the dynamics of the formation of partnership. He insists that national foreign policy remains the major center of power, but his realistic examination and analysis of coordination exercises of the European community are valuable. Simonian sheds new light on the emergence and development of the Franco-German nexus of power in global politics and the compelling role of the world's largest trading bloc in that evolution.

PIERRE-HENRI LAURENT
Tufts University

D. C. PECK, editor. *Leicester's Commonwealth: The Copy of a Letter Written by a Master of Art of Cambridge (1584) and Related Documents*. Athens: Ohio University Press. 1985. Pp. xii, 323. $35.00.

Elizabeth I, the most popular of English rulers, bestowed her confidence on Robert Dudley, earl of Leicester, a man who was in his lifetime the most hated of contemporary politicians and whose posthumous reputation is deeply shadowed. That tainted reputation results in no small part from an anonymous publication, issued in the last years of Leicester's life, that brings together within one cover many of the hostile tales and rumors that had circulated during his career.

The work printed here, published in 1584, was the product of a group of exiled Catholics who had backed the abortive Anjou marriage. A dialogue in form, the book incorporates several goals: a full-scale vilification of Leicester, including every misdeed in the calendar, even murder; a proposal for general religious toleration; and a defense of the succession claims of the Stuarts.

The pamphlet certainly succeeded in its first goal, for most posthumous judgments of Leicester reflect at least some elements of its attack. The arguments in favor of toleration were serious and raised for the first time in England a call for mutual respect among all English Christians and for an end to coercion in religious matters. The defense of the Stuart claim was not a new theme, but the presentation made by these authors carefully set out the persuasive legal arguments with attention to detail and accuracy. Those arguments did little for Mary but may well have helped establish the plausibility of James's claim a decade later.

D. C. Peck's editing is careful, helpful, and copious. The introduction gives a full account of the probable authorship, the setting of the tract's publication, and its immediate reception. Peck makes a sane assessment of the consequences, both long- and short-term, and includes a final section that thoughtfully analyzes the problems of any historical evaluation of Leicester, a task that yet lacks a full-scale treatment.

WALLACE T. MACCAFFREY
Harvard University

LAURO MARTINES. *Society and History in English Renaissance Verse*. New York: Basil Blackwell. 1985. Pp. viii, 191. $24.95.

In this brief study Lauro Martines, a well-known and well-respected historian of the Italian Renaissance, argues that historians of Tudor and Stuart England should turn their attention to integrating social and literary history in general and to the social study of poetry in particular. Martines, who employs the term "world" to refer to the social and historical setting of the poem and the term "poem" to refer to particular poems, attempts to "hurdle the gap between poem and social world" in order to bring the two together.

The first and last portions of the book seek a theoretical position for integrating the two fields of study, which experienced relatively little interchange during the decades in which literary scholarship was dominated by the "new critics." Indeed, Martines's study is largely built on presenting the work of a new group of more historically sophisticated literary historians. Interestingly, Martines relies far less on the work of English intellectual or

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social historians. Martines is dependent on the literary scholars for two reasons. The first is that he is far more knowledgeable and familiar with the work of literary critics and literary historians than he is with the work of English intellectual and social historians. The second and perhaps more important reason is that, although literary scholars have been moving toward historical and contextual studies, English social historians have to a large extent avoided topics dealing with culture, particularly elite culture. English intellectual historians have become so specialized in the history of science, religion, and so forth that they have tended to avoid interdisciplinary studies.

It is thus not surprising to find that the portion of his book devoted to relating the poet to the "world" is far richer than that relating the "world" to the poet. Martines has been quite successful in demonstrating that recent developments in the field of literary history have enabled scholars to treat literature as part of society. The work he pleads for is thus well underway—if not by historians. The portion of the work that attempts to relate the world to the poet is less satisfactory, largely because relatively few historians of the period have concerned themselves with this problem. Martines does point out that the important subject of patronage has recently attracted considerable attention, but he does not pay sufficient attention to the impact of printing or to education. For example, it might be worth investigating whether educational values and modes of instrumentation (for example, the widespread use of Erasmus's *De Copia*) might have helped produce both poets and audiences trained in and appreciative of poetry. The university is also ignored as part of the literary world.

The primary value of this book for historians is to bring them abreast of developments in literature that bear on historical studies. It remains to be seen whether Tudor and Stuart historians will find literature an appropriate object of historical inquiry.

BARBARA SHAPIRO
University of California, Berkeley

DAVID HOWARTH. *Lord Arundel and His Circle*. New Haven: Yale University Press. 1985. Pp. 256. $45.00.

David Howarth's wide-ranging study of Thomas Howard, earl of Arundel, records the process of Arundel's collecting of drawings, paintings, sculpture, and antique inscriptions from first inspiration to final purchase. Although the earl of Clarendon thought Arundel knew little about what he collected ("he was only able to buy them, never to understand them" [p. 219]), Howarth shows that Arundel was highly knowledgeable and, from his early journeys to Europe in 1612 and 1613, modeled himself after the great Renaissance patrons.

From his Howard and Lumley relatives Arundel derived his interest in antiquities, some of his books, and some of the scholars in his circle. From his marriage to Aletheia Talbot he acquired the wealth with which to build their collections. Although Howarth does not attempt to reconstruct Arundel's magnificent holdings, he does analyze his relations with artists, especially Peter Paul Rubens, Anthony Van Dyke, Inigo Jones, and Wenceslaus Hollar; his ties with English antiquaries such as William Camden, Robert Cotton, and John Selden; and his patronage of the writers Francis Junius and Henry Peacham. Howarth describes in detail the work of Arundel's agents, such as William Petty, and the English ambassadors abroad who helped him secure Graeco-Roman sculpture and inscriptions, the Pirckheimer collection of books, and drawings by Dürer, Leonardo, and Michelangelo. Arundel opened his galleries to English and foreign visitors, maintained extensive correspondence with other European patrons, and had his collections reproduced in order to secure his own fame and to shape English culture. As a young man, he had traveled on the Continent with Inigo Jones; at the end of his life he provided John Evelyn with detailed instructions of what to see in Italy. His bid for immortality was short-lived, however, and parts of the collections were sold in the 1640s as the earl wandered in exile.

Howarth's book goes well beyond Arundel's published letters to draw on manuscripts in Italian as well as English archives; one item not noted is the Sotheby catalogue of 1927, which records the sale of part of the Arundel library and provides a fascinating picture of Howard holdings in the early seventeenth century. The book is not sure-footed, however, when the author ventures beyond art history. Thus, he explains Arundel's correspondence with the royal favorite, Robert Carr, earl of Somerset—"so despised and corrupt a figure" (p. 43)—as a result of their mutual love of pictures and the need to flatter the current favorite, omitting the crucial fact that Somerset was closely allied politically and by marriage to the Howards. Howarth errs in calling Sir Robert Cotton a "radical politician" (p. 24) who "towered over the Jacobean intelligentsia, as Johnson was to dominate the eighteenth-century coffee house" (p. 23), in stating that the Society of Antiquarians [*sic*] was founded in 1573 and resumed meetings in 1615, and in putting forward an oversimplified view of the origins of the Royal Society.

Nevertheless, Howarth firmly places his study of early seventeenth-century English collecting and English taste in the European setting where it belongs. The book is beautifully illustrated with both color plates and black and white reproductions of

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portraits of the earl and countess of Arundel, their contemporaries, and their collections.

LINDA LEVY PECK
Purdue University

BARRY REAY, editor. *Popular Culture in Seventeenth-Century England*. New York: St. Martin's. 1985. Pp. 318. $29.95.

The low-budget production of this book (off-set from typescript) suggests its interim character; it is primarily a synthesis of existing studies. But the essays focus on an important period that saw the substantial divorce of elite and popular culture, and they share a common tone in emphasizing the mutually supporting, rather than the "hegemonic," relation between the different levels.

Barry Reay strikes a slightly different note, however, while introducing the familiar concepts of the field. He makes a play for our sympathies with the oppressed yet offers the rather contradictory assurance that, being able to manipulate elites through cultural tradition, they were not so helpless or dumb as we might think. Even Reay, though, rejects the application to England of Robert Muchembled's thesis that religious reformation and the developing nation-state destroyed a golden age of popular culture and the pleasures of the poor. Some of the other authors are so disloyal as to hint that popular culture was essentially dependent and withered after the withdrawal of elite participation.

Peter Burke's article of 1977 on the London scene is reprinted and reminds us of the unique position of the political capital but does not sufficiently explore the question of whether, despite high rural migration, this urban culture should be lumped with rural custom. Emphasizing the importance of printing and religious dissent in a changing scene, Jonathan Barry's article on Bristol views civic culture on a smaller scale.

In his second essay Reay ignores his generic definition of religion to concentrate on Christianity. He tends to view popular religion in terms of its deficiencies with reference to official standards. Like the others, he does not follow up Keith Thomas's use of anthropological approaches toward recreating a popular cosmology (if a perspective independent of Christian or classical views really existed) or toward penetrating the cultural grammar of the poor. Bernard Capp shows the relation between various forms of printed and oral literature. He ignores any category of "scribal culture," perhaps because the lower classes jumped from the oral stage directly to print.

The essays by Martin Ingram on sex and James Sharpe on law deal with social history and establish the incidence of various activities and their social location rather than show their meaning for the participants. Ingram's contribution on skimmington (or charivari) and Buchanan Sharp's on riots report research that reinforces familiar theories on these matters. All of the essays do a good job of emphasizing the interpenetration of cultural levels, sometimes to the point of suggesting that "popular culture" was not a self-sustaining enterprise.

JOHN SOMMERVILLE
University of Florida

BARRY REAY. *The Quakers and the English Revolution*. Foreword by CHRISTOPHER HILL. New York: St. Martin's. 1985. Pp. xii, 184. $22.50.

The scope of this book is less magisterial than its title might suggest. Barry Reay—one of the best of the younger historians now working on early modern English popular culture—has not attempted a full-dress history of early Quakerism, still less another interpretation of the English revolution. Rather, he has given us a series of linked essays, exploring the social composition of the Quaker movement during its first decade and assessing its political impact at the end of the Protectorate. Reay persuasively argues that the fear and hatred aroused by the Quakers contributed powerfully to the Restoration of Charles II.

Reay first provides an estimate of the social composition and geographical distribution of early Quakerism, supplementing and amending previous scholarship with his own research. The early Quakers, it appears, were reasonably representative of the middle range of English society. Quakerism was certainly not a movement of the poor; if anything, Quakers tended to be better off than the general population. Originating in the north of England, the movement spread widely, albeit thinly, across England within the first generation.

But, as Reay makes clear, the general reaction to the Quakers—despite occasional instances of sympathy from the "unenlightened" majority—was fear and loathing. And no wonder. They were, from the point of view of the ordinary English man or woman, a thoroughly obnoxious lot—strident, frighteningly eccentric, insufferably arrogant. They ran naked through the streets, heckled ministers, and threatened apocalyptic vengeance. They were widely assumed to be witches or, at the very least, clandestine Jesuits. They brought plagues and famines on the land.

Quakers, of course, attacked tithes and refused to acknowledge social distinctions. They used the familiar "thee" to social superiors and refused to doff their hats. They carried on, if only in the symbology of everyday behavior, the radicalism of the defeated Levellers. But, as Reay demonstrates, the Quakers

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were hated not only by the rich, whose privileges they threatened, but also by the common folk, whose customs and values they outraged. One of Reay's great strengths is his recognition of the depth of popular conservatism: he is not shackled by the assumption that only insurgent values are authentically "popular."

Reay's achievement in this graceful and concise book is to make both the early Quakers and their persecutors humanly intelligible; he writes with sympathy but without illusion.

WILLIAM HUNT
St. Lawrence University

BERNARD COTTRET. *Terre d'exil: L'Angleterre et ses réfugiés français et wallons, de la Réforme à la Révocation de l'Edit de Nantes, 1550–1700.* Foreword by EMMANUEL LE ROY LADURIE. (Collection Historique.) Paris: Aubier. 1985. Pp. 337. 125 fr.

This work is doubly welcome. It gives us a French perspective on the England of the later Tudors and Stuarts and does so by studying a fascinating and undeservedly neglected subject: the French-speaking Protestant refugees who crossed the Channel.

Focus on the refugees allows comment on such wide-ranging aspects of English society as Protestant expectations under Edward VI, the nature of absolutism under James I, the development of Arminianism and Socinianism, and the revolutions of the 1640s and the 1680s. At the same time, Bernard Cottret seeks to comprehend the world of the refugees themselves and asks whether they have a definable identity as a minority. The study of *mentalités* requires a comprehensive knowledge of the sources, coupled with a subtle mind to tease out their nuances. Cottret is well equipped for the task, widely read, capable of giving his sources an unexpected gloss, and careful to define the problems confronting the historian. His frequent use of "I" may irritate American and English readers, who will also bemoan the lack of an index (although footnotes and a bibliography are included). But they should not be deterred from consulting this perceptive and well-informed book, the first written in French on the subject in this century.

Inevitably, there are omissions. The Channel Islands, important to the refugees in many respects, are neglected, and Devon, a refugee center in the 1680s, is ignored. Perhaps because he has consciously set out to write the history of these refugees "for men of our own times" (p. 58), Cottret undervalues the importance to the Huguenots of their specific form of church discipline. More might have been said about the social position of lay elders, who were often able to influence the employment prospects of church members as well as to supervise their morals. A few important recent works in English have escaped the author's attention: Patrick Collinson's *Archbishop Grindal* (1979), the history of the French churches in England in the 1640s and 1650s in volume 54 of the Huguenot Society of London Quarto Series, and a number of valuable articles in the *Proceedings* of that society. Cottret lost an opportunity by ending his book in 1700; as he shows, by far the largest number of refugees came to England during Louis XIV's reign, yet no book in French deals primarily with that exodus.

After a masterly survey of the background to the revocation of the Edict of Nantes, the introductory essay by Emmanuel Le Roy Ladurie makes a series of comparisons between the situation of Huguenots in France and that of Catholics in England—comparisons that are interesting but not always wholly convincing, since England witnessed no Massacre of St. Bartholomew, no wars of religion, and no *dragonnades*. The argument may be better founded in relation to Ireland, but, in any case, it is stimulating.

Implicit in both parts of this thought-provoking book is that religion was much more important to the men and women of the late seventeenth century than modern historians have tended to let us believe. For this and other insights, one hopes that Cottret's monograph will soon be translated.

ROBIN GWYNN
Massey University

J. M. BEATTIE. *Crime and the Courts in England, 1660–1800.* Princeton: Princeton University Press. 1986. Pp. xxiv, 663. Cloth $72.00, paper $19.95.

In 1883 James Fitzjames Stephen published his three-volume history of English criminal law. As far as the post-medieval period is concerned, virtually nothing else of importance on the working of the system of criminal justice appeared until 1948, nearly three-quarters of a century later, when the first volume of Leon Radzinowicz's *History of English Criminal Law and Its Administration from 1750* initiated modern scholarly investigation into the subject. Since then, a remarkable number of distinguished works have been published, including four more books by Radzinowicz. It is no exaggeration to say that this has both transformed understanding of how the system operated and, like all the best historical work, raised new questions and puzzles. One factor in particular has injected an element of lively controversy into attempts to explain the social meaning of the criminal law of the period: the application of Marxist analysis to the sources, such as in E. P. Thompson's *Whigs and Hunters* (1975), and the collection of essays published as *Albion's Fatal Tree* (1975). In consequence, the subject of criminal

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law has come to function as a testing ground for the validity and explanatory value of such an analysis, one that has yet to be applied to any other area of English legal history with comparable elegance and force.

J. M. Beattie's book is both the latest addition to this body of writing and surely as important a piece as has yet appeared. Indeed, its breadth and balance are such that it may well shift scholarly interest to other periods, in particular the nineteenth century, about which much is known but perhaps not so much understood. Beattie's account is primarily founded on a meticulous study of the considerable body of surviving records of assizes and quarter sessions for the county of Surrey, a county that included both an urban area of expanding London and a rural area in which patterns of crime and law enforcement might be expected to differ. He has also used the Sussex court records, though less extensively. In studying the archival material, which forms the basis of his book, Beattie has, of course, employed a wide range of other material—for example, pamphlet reports of criminal trials and correspondence related to the granting of pardons—and made full reference to the work and theorizing of other scholars. But the strength of his work lies in the careful, detailed, and always-balanced study of the source material. Hence, his theoretical analysis never becomes divorced from evidence.

The first part of his book concentrates on the historical interpretation of prosecutions as a recorded and quantifiable phenomenon and on the problematic relationship between the statistical evidence and the realities of social behavior to which the records provide, at best, an indifferent guide. What relationship, if any, can be detected, for example, between the frequency of prosecution of violent offenses and the actual incidence of violence in society? Here the problems that are familar today in the search for the dark figure of "real" unprosecuted crime exist in a more acute form, for we have little to use to supplement the court records. The conclusions Beattie reaches are necessarily put forward with diffidence; indeed, I doubt whether his claim that a real relationship between the number of prosecutions and the real level of crime both exists and is discoverable will convince all readers.

The second part of the book deals with processes through which the criminal law was adminstered—in particular the form of the trial itself and the system of punishment and pardon that was its end product. Beattie displays his mastery of the materials most impressively in this part of the study, particularly in his account of the connections between changes in the form of criminal sanctions and changes in the whole process by which the system was operated and administered. During the period covered by this study, transportation evolved as a workable form of secondary punishment and was, in its turn, joined and eventually superseded by imprisonment, the typical sanction of modern criminal law. The period is one during which the system of criminal justice was transformed into something not wholly unlike what exists today; no better account of that transformation than what this book provides could be asked for.

A. W. BRIAN SIMPSON
University of Chicago

JOHN FEATHER. *The Provincial Book Trade in Eighteenth-Century England*. (Cambridge Studies in Publishing and Printing History.) New York: Cambridge University Press. 1985. Pp. xvi, 176. $39.50.

John Feather aims to tell the reader "how those living outside London obtained their books" (p. x), and this he has done in a well-organized and concise summary of the provincial book trade. Working from scattered and limited sources, he has assembled a good overview of books and booksellers outside London in the eighteenth century.

The emphasis of the book, quite rightly, is on distribution. Although local authors, printers, and publications naturally take priority in local histories, a work such as this, examining the economics of the book trade, has to concentrate on the commercial aspect. That story is also intrinsically interesting in the context of the economy and society of eighteenth-century England.

The natural starting point is the explanation of London's monopoly in the seventeenth century and of the end of licensing in 1695. Various measures of control were attempted thereafter, but the Londoners' efforts to assert legal monopoly were probably redundant, for their privileged economic position was security enough against loss of power. For introductory purposes the author divides the eighteenth century into three periods (chap. 2). Following a topical organization, the remaining chapters discuss the market for books, the distribution system, the bookselling business, and the printing office. Assorted original sources are appended. The book is also illustrated with some samples of eighteenth-century advertising taken from the provincial papers, which were vital corollaries to the book trade. There are two indexes: one for the provincial trade (persons and places) and one for general subjects.

One of the most interesting aspects of this book is the picture one develops of the provincial bookseller (*cum* stationer, patent medicine dealer, and news agent). A few prospered sufficiently from selling books, but most were also retailers, and some were distributors of duty stamps as well. Feather has done a good job of piecing together enough evidence for

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a profile, and, if it is modest in quantity of documentation, it is fascinating in detail.

The author's work was complete before the *Eighteenth-Century Short Title Catalog* became available. Also, in the discussion of legal aspects of the book trade, the role of Scottish, Irish, and colonial "pirate" editions is noted, and one would like to know how much influence these remoter "provinces" may have had on the trade. These, however, are minor complaints about a book that will be valued for some time as a contribution to our understanding of bookselling. The brief conclusion raises many interesting issues that it cannot resolve. Indeed, it reads like a proposal for future study. I look forward to the sequel.

RICHARD S. TOMPSON
University of Utah

GAIL MALMGREEN. *Silk Town Industry and Culture in Macclesfield, 1750–1835*. Hull, England: Hull University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1985. Pp. xii, 259. $18.50.

The historiography of the English Industrial Revolution has changed dramatically since the early twentieth century. Investigators such as J. L. Hammond and Barbara Hammond once wrote sweeping analyses of the entire industrial phenomenon and often tried to compensate for a lack of source material by a vigorous style and a clearly defined point of view. Gail Malmgreen's study of the Cheshire town of Macclesfield is different. Her book is based on detailed research into a small corner of industrial England, and it is similar to other recent efforts to advance our knowledge of the subject by means of painstaking scholarship on particular topics.

This is all to the good. A generation ago university students of the Industrial Revolution were saturated in the "optimist" versus "pessimist" debate that was sometimes expressed in generalities. But, through studies such as this, we can leave imprecision behind and proceed to deal with the reality in each local area. Malmgreen notes that the study of local history can remind us of the diversity that lies behind the generalizations of social theorists.

Grounded in relevant primary and secondary sources, Malmgreen's book discusses issues such as employer-employee relations, the standard of living, protest, and religion. In a judicious conclusion Malmgreen asserts that the gains "if not spectacular, were not negligible" (p. 184). The losses, which were often intangible, are harder to assess. More prosperity, mobility, and literacy were matched by overcrowding, an oppressive work discipline, and anxiety.

This applies to Macclesfield; workers in other industrial towns certainly had their own experiences. Macclesfield had an economy based on silk, but silk was not the major textile in the English Industrial Revolution. Centers of cotton and wool production would have different histories and so would iron-manufacturing and coal-mining localities. Histories of some of these places have recently been written or are in the process of composition. Eventually a synthesis based on monographs such as the one under discussion may be produced. The resulting work would be just as stimulating and far more accurate than was the work of the pioneers in this field of history.

This book has something for the urban historian, the labor historian, the social historian, and for the political historian also (Macclesfield was unrepresented in the House of Commons until the passage of the Reform Bill of 1832). The treatment is fairminded and the author's writing is lucid, although her field is one in which the reader frequently encounters a clumsy style. In short, this is a solid, intelligent work that will have to be considered by all scholars of the English Industrial Revolution.

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JOHN BELCHEM. *"Orator" Hunt: Henry Hunt and English Working-Class Radicalism*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 304. $39.95.

Among the leaders of early nineteenth-century British radicalism, "Orator" Hunt has had a particularly bad press. Attacked harshly by contemporaries and characterized as a demagogue even by the historian Edward P. Thompson, Hunt has had few defenders. His vanity, scurrilous attacks on other reformers, and seeming lack of principle have made him an easy target for critics.

John Belchem's important biography of Hunt—"an essay in recovery, not in revisionism" (p. 2)—substantially reverses this verdict. Hunt emerges as a major spokesman for working-class aspirations. A gentleman farmer and speculator, he gained a reputation as an extreme radical in the years after 1815—a time when the emergent working class was suffering from the effects of economic upheaval and rapid industrialization. Hunt sought to temper these changes. He believed that a democratic suffrage would bring about necessary economic and social reforms. In his oratory he drew on a constitutional tradition of legality that was somewhat different from the emphasis placed on natural rights by Richard Carlile and other reformers. He also rejected the pragmatism of William Cobbett, another

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leading spokesman for radicalism. At Spa Fields (1816–17), Peterloo, and elsewhere Hunt repeatedly made the case for a redistribution of political power. Occasionally his speeches countenanced violence. But, as Belchem demonstrates, they were marked by a consistency that carried into the 1830s and, after Hunt's death in 1834, through the Chartist period and beyond.

This study of Hunt is especially significant because of its emphasis on context. Hunt is depicted as a political leader whose strength was rooted in the "mass platform." His speeches were, admittedly, a rather simplistic critique of "old corruption." Yet, he had a genuine vision of a better society, in which political and economic power was to be more equitably shared. In the 1820s Hunt gave effect to this vision in his Great Northern Union based in Manchester. He also had a substantial following in London, where for many years he led the fight for the reform of City politics. In December 1830, after assisting in the revival of radical agitation, he was elected to represent Preston in the House of Commons. For more than a year he opposed the various Whig measures for parliamentary reform in the belief (not shared by many other reformers) that these were regressive and inadequate to working-class needs. To some extent, Hunt's poor reputation derives from his ineffectiveness as a member of Parliament.

Belchem's biography is unlikely to be surpassed. He has burrowed deeply in the archives and has read as much about Hunt as seems possible. If at times he is quick to exonerate Hunt for his personal failings, and even too apt to endorse his intemperate language, this is perhaps understandable. Nineteenth-century radicalism embodied many traditions and leaders. If Hunt remains an unlikeable man, this must not detract from our recognition of his importance as a radical leader.

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PHILIP PRIESTLEY. *Victorian Prison Lives: English Prison Biography, 1830–1914.* New York: Methuen. 1985. Pp. xii, 311. $25.00.

Philip Priestley has woven together hundreds of personal narratives of English prisoners between 1830 and 1914, and he has blended them into a seamless, densely detailed description of prison life in Victorian England. Priestley allows the prisoners to speak for themselves, and we can follow them from arrest to release: their lives in prison, the numbness of their daily routines, the inanity of their makeshift work, and the pettiness of the discipline to which they were subjected. Prominent, but not foremost, we follow the years in prison of the American brothers, George and Austin Bidwell, convicted in 1873 in a sensational trial for fraud against the Bank of England and sentenced to "penal servitude for life." George entered prison a dark-haired young man and was released for medical reasons in 1887 a gray-haired cripple. Notables such as Grover Cleveland, Mark Twain, Harriet Beecher Stowe, John Bright, and Randolph Churchill petitioned the Home Office for the release of Austin, who finally left prison in 1893. Whether small-time crooks, murderers, Fenians, Chartists, or suffragists, prisoners who, like the Bidwells, left us personal prison accounts all hated the prison system, which imposed inflexible discipline and arbitrary punishment for the most trifling infringement of prison rules. Priestley did not intend to write a broad social history of Victorian England as it related to the development of the English prison system. His book also does not show traces of economic causation and social analysis found in the studies of prisons by Michael Ignatieff and Michel Foucault. Priestley has a much narrower focus; he gives us a collective account drawn from prisoners' experiences. He informs us in that context how the English prison system was transformed from a confusing multiplicity of prison administrations into a central prison system in 1877, of which Sir Joshua Webb and Sir Edmund DuCane were the chief architects. We learn much more, however, about such arcane matters as prison diet, cell furniture, pets (George Bidwell trained a rat to stand upright "like a soldier"), the prison chapel, medical care, female warders, prison violence, the library, the hospital, and much other information that can bring us closer to that prison experience. This abundant information makes us aware that the prison system that emerged in Victorian England and that was intended to promote a moral reformation of its inmates through enforced silence and separation, regimentation, hard labor, and strict discipline became just another example of disappointed ideals that had gone awry.

This book, which is solidly researched and fully documented by the author, a former probation officer, prison welfare officer, and university researcher, has the added virtue of being quite readable. Priestley has gathered information about Victorian prisons that cannot be found in any other text. This book will have decided value for students of criminology and penology and for all interested in the social history of Victorian England.

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The Correspondence of Charles Darwin. Volume 1, *1821–1836.* New York: Cambridge University Press. 1985. Pp. xxix, 702. $37.50.

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This first volume of the letters of Charles Darwin has been reviewed enthusiastically in the scientific and the literary press, and there is little need to repeat the praise that has been heaped on the editors for the quality of their work. Here is a record of all the available letters written and received by Darwin up to his return from the voyage of the *Beagle*. The second volume—due in late 1986—will cover the years in which he worked out his theory of natural selection. Both will provide valuable material for scholars investigating these early developments, although those of us whose interests lie in the later part of Darwin's career may have to wait for some time. Up to twenty volumes are projected, to be spread over a decade or more.

My anticipation is, however, tinged with an uneasy feeling that the commitment of so much scholarly energy to the work of a single figure may not be healthy for the future of a small discipline such as the history of science. Darwin's name still attracts public attention, and the profession cannot be blamed for exploiting this fact. The letters will be useful to others besides Darwin scholars, of course, since they provide cross sections through a series of networks of correspondence illustrating the scientific and social worlds of nineteenth-century Britain. But will these cross sections be truly representative of the time, since they are centered on a figure whose ideas remained controversial throughout his career? As a specialist in the study of non-Darwinian evolutionary theories, I am acutely aware of the extent to which such ideas predominated in late nineteenth-century biology. The subsequent revival of natural selection as the foundation of modern Darwinism has encouraged a Whig history approach that ignores all those biologists who did not lie on this "main line" of scientific development. Will it not be more difficult to challenge this interpretation after the completion of the publication of Darwin's letters, providing much easier access to material that will inevitably concentrate on those sympathetic to the Darwinian case? Such a project embodies our current thinking on who was the central figure in the debate—and may then make it more difficult for future historians to evaluate that interpretation.

I certainly do not want to see this project stopped. I admit that Darwin was important, and I am as anxious as anyone to see the letters from the later years. But I do think that the scholars who initiate such projects should begin to think more carefully about whether the resources they command will be used in the most effective way. Hero worship provides an easy way to gain attention but is a dangerous foundation for scholarship. One has to ask if the future might have been better served by a spread of activity that would produce a more balanced cross section of the vast amounts of manuscript material that could be published.

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G. R. ELTON. *F. W. Maitland*. New Haven: Yale University Press. 1985. Pp. viii, 118. $15.00.

The purpose of this book is to deliver F. W. Maitland from the adulation with which he has been surrounded and to evaluate his work and influence afresh. How great a historian was Maitland? This is the question that G. R. Elton wishes to answer.

A chapter devoted to a general discussion of Maitland's historical writing emphasizes that he worked directly from archival materials; Elton remarks that proper knowledge of the sources is not to be taken for granted, then or since. Other distinguishing features of Maitland's writing are indicated: with rare exceptions, he limited himself to the study of English law from 1066 to the reign of Edward I; his major works were frequently based on the examination of a single source; he wrote analytical rather than narrative history; finally, his sheer enjoyment of both historical research and writing is evident in his books and articles. Aware that historians do not produce final or absolute truths, Maitland offered only provisional and tentative conclusions, which have nevertheless "created lasting orthodoxies" (p. 32). Elton then discusses an account of the views of historians who have questioned these "orthodoxies," from James Tait's criticism of the "garrison theory" of the English borough to S. F. C. Milsom's argument that Maitland misconstrued the relative importance of royal and feudal justice in twelfth-century England. Elton's moral is that Maitland occasionally went astray, falling victim to the teleological heresy (to which historians are prone) or constructing persuasive syntheses that were too tightly organized and dependent on a single source. In short, even Maitland sometimes failed to hold fast to a thoroughgoing empiricism.

The most substantial chapter of this book presents detailed studies of four of Maitland's works, *Memoranda de Parliamento* (1893), *Roman Canon Law in the Church of England* (1898), *English Law and the Renaissance* (1901), and "The Anglican Settlement and the Scottish Reformation," a chapter in the second volume of the *Cambridge Modern History*. In Elton's opinion *English Law and the Renaissance* must be considered a failure, because Maitland accepted the conventional wisdom about the revival of Roman law in the early sixteenth century and the efforts of Henry VIII to establish a despotism. In contrast, the chapter in the *Cambridge Modern History*—another of Maitland's rare excursions outside of medieval history—is adjudged an accurate ac-

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count of the settlement of 1559, although a bit weak on the confusing Scottish scene. In discussing *Memoranda de Parliamento*, Elton explains that Maitland, through his analysis of the Parliament roll of 1305, radically revised the accepted Stubbsian theory of the functions of the medieval Parliament. Elton suggests that Maitland's conclusions, corroborated as they have been by the work of H. G. Richardson and G. O. Sayles, could profitably be applied to the history of Parliament from the Tudor period onward. In *Roman Canon Law in the Church of England* Maitland was again forced to take issue with Stubbs, this time on the question of whether church courts in medieval England considered themselves bound by the decisions of Roman canon law. Maitland's reply was a resounding affirmative, unpalatable to Anglicans who preferred to think that the pre-Reformation church had a certain independence from Rome. In this matter also, Maitland's interpretation has been confirmed by subsequent scholarship.

How great a historian was Maitland? One cannot quarrel with Elton's affirmation that Maitland, though capable of error, was indeed a great historian. Elton seems bent, however, on claiming that Maitland was the founder of professional history in England. This is an exaggeration. In a generation of historians that included C. H. Firth and T. F. Tout, Maitland was not alone in recognizing the need for archival research. Elton argues that Maitland was "quintessentially a university professor" (p. 98), that he regarded himself as part of an international community of historians, that he was concerned with training future scholars, and that he organized the Seldon Society. These attitudes and activities do not make Maitland unique; rather, they were characteristic of the first generation of English historians who pursued careers within the institutional setting of universities.

What sets this short book apart from other studies of Maitland is lucid, at times sprightly, exposition combined with incisive critical assessment. Moreover, Elton has succeeded in imparting a sense of the excitement to be found in reading the work of a great historian. That is an infrequent and noteworthy achievement in historiography.

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MICHAEL BALFOUR. *Britain and Joseph Chamberlain.* Boston: George Allen and Unwin. 1985. Pp. xiii, 311. $35.00.

Michael Balfour begins this "times and life" study of Joseph Chamberlain with two general chapters dealing with the economic and radical background of modern Britain. Chapter 3, "'Young Joe': 1836–65," deals with Chamberlain's nonconformist London family, his education, his relocation to Birmingham and rapid success in business, and his short-lived, happy first marriage. Chapter 4, "'Brummagem Joe': 1865–80," deals with his election as Birmingham's mayor, his establishment of municipal ownership of the gas and water companies, his extensive demolition of slum dwellings, his campaign to amend the Forster Education Act, his organization of the Birmingham "caucus" and of the National Liberal Federation, and his election to Parliament.

Chapter 5, "'Radical Joe': 1880–85," begins with his appointment to W. E. Gladstone's second cabinet and discusses his unhappy relations with his ministerial colleagues and his advocacy of an "unauthorized program" and demagogic public speeches. Chapter 6, "'Judas Joe': 1880–85," discusses Chamberlain's unsucessful advocacy of limited self-government for Ireland, his part in the fall of Gladstone's second government, his suspicious role in the divorce suit that discredited Sir Charles Dilke, his agreement to join the third Gladstone cabinet, his opposition to Home Rule, and the tragic consequences of Chamberlain's resignation for Ireland and his social reform program.

Chapter 7, "'Pilgrim Joe': 1886–95," discusses the problems of the Liberal Unionists in supporting a Conservative government, the abortive negotiations for Liberal reunion, Chamberlain's role in the judicial inquiry into Parnellism and crime and the O'Shea divorce case, his visit to the United States to negotiate a fishing treaty, and his appointment as colonial secretary in Lord Salisbury's third cabinet. Chapter 8, "'Pushful Joe': 1895–1902," deals with Chamberlain's complicity in the Jameson raid, his opposition to French and Russian expansionism, his "amateur" negotiations for a German alliance, and his role in provoking the Boer War.

Chapter 9, "'Imperial Joe': 1902–14," discusses Chamberlain's dissatisfaction with Arthur Balfour's Education Act, his attempts to strengthen the ties of empire, his second "unauthorized program," the elections of 1906, the Valentine's Day letters, and his paralytic stroke. Balfour concludes with "Joe and his World," a chapter in which he offers a critical analysis of Chamberlain's character and questions the relevance of his ideas to the problems of late Victorian and Edwardian Britain.

The author, as he admits in the preface, has relied on printed, not manuscript, sources for his material (p. xiii). But he has consulted the standard biographies, memoirs, collected speeches and letters of Chamberlain and of contemporary politicians, as well as numerous period studies, regional histories, monographs, and articles in scholarly journals and contemporary publications, which he cites in the footnotes at the end of each chapter. It is evident

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that he has profited from his previous research for his well-known biography of Emperor William II and also that he has given careful thought to the economic and other problems that contributed to the decline of contemporary Britain. Balfour's new book, although not likely to be definitive because of its vast scope, represents a fresh approach to Chamberlain and his era and will doubtless interest the student as well as the general reader.

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R. P. T. DAVENPORT-HINES. *Dudley Docker: The Life and Times of a Trade Warrior*. New York: Cambridge University Press. 1984. Pp. xii, 295. $44.50.

The name of Dudley Docker will not be known to most readers of the *AHR*; indeed, even to students of British business history since 1880 this portrait of a Midland manufacturer, whom R. P. T. Davenport-Hines describes as one of the most powerful businessmen in Europe, will come as a surprise. Whether, in effect, Docker was as important as he or Davenport-Hines claims is, however, debatable. As leading figure in such major companies as Vickers, Birmingham Small Arms (BSA), Daimler Motors, the Midland Bank, and the forerunner of the General Electric Company and as chairman of Britain's major rolling stock company, Docker influenced the history of British industry considerably, as the fluctuating fortunes of these companies recounted by Davenport-Hines shows. Docker was a mainspring of the merger movement in the Midlands, the heartland of the British metal industries in the 1900s, although the rhetoric of rationalization proved easier to articulate than to translate into effective industrial integration. In part, this is explained by a lesson learned in the process of merger: that industrial finance could be far more profitable than manufacturing, for it largely avoided the complex problems of reorganization and coordination that successful rationalization implied. Such a lesson and the financial wheeling and dealing that became his forte negated his grander aspirations to rejuvenate British industry, as the later history of Vickers and BSA was to show.

Turning away from the uncertainties of the entrepreneurial approach to industrial regeneration, Docker sought the support and cooperation of other large manufacturers in the Midlands to rebuff the German threat to British industrial hegemony. When World War I came, he regarded it as a crisis of modernization that offered Britain a final chance to regain economic leadership by victory and a carefully prepared economic reconstruction based on trade protection and a united British empire. He saw corporatism as the key to industrial reconstruction, and to this end he founded the Federation of British Industries (FBI) and the British Trade Corporation (BTC)—the latter to provide support for business reconstruction and to continue the trade war in peacetime. The closer involvement of businessmen in government to promote the war effort chimed with the conviction expressed by the Business League, in which Docker was a major figure, that only business could provide the government with the sound management that would retrieve Britain's economic preeminence. The war, however, revealed the weakness of this argument, and the continuing cultural differences between businessmen and professional politicians—and between businessmen themselves and especially the conservative opposition from pillars of the City of London—destroyed Docker's belief that corporatism was the panacea. Tired of, or bored with, running factories in Britain under adverse economic conditions and of attempting to translate the popular parrot cries calling for "business government" into political reality in an unsympathetic social climate, Docker had withdrawn from the FBI, the BTC, and Vickers by 1920. Next he turned his attention to multinational finance.

This excellent scholarly study of the life and times of Docker is as enjoyable to read as it is illuminating and, from the British standpoint, depressing. It presents a paradox of an extremely able, assiduous, and articulate businessman and patriot, a "trade warrior" whose vision of industrial renaissance was vitiated in part by his predilection for the accumulation of personal wealth by the quickest means he knew: by subordinating manufacturing to money making.

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JOHN SCOTT and CATHERINE GRIFF. *Directors of Industry: The British Corporate Network, 1904–76*. Cambridge: Polity; distributed by Basil Blackwell, New York. 1984. Pp. 226. $39.95.

This book should promote one of John Scott and Catherine Griff's objectives: "a closer integration of economics and sociology" (p. 187). It shows how graph theory may contribute to an understanding of British economic history as seen from a sociologist's perspective. The authors analyze the composition of the board of directors of two hundred leading nonfinancial corporations and fifty leading financial corporations in 1904, 1938, and 1976. They focus on the nature of the morphogenesis of these sets of decision makers over the period to identify the prime characteristics, as set in a societal framework, of the changing nature of the corporate

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power elite. Features such as "connectivity" of directorships, "density" of directorships, "clusters," "cliques," "components" of directorships are obtained through feeding the data, as classed by societal affiliation (kinship, primary business affiliation, and so on) through the GRADAP program, developed by the University of Amsterdam's Technical Center. These features are analyzed and compared with results from similar works published in the United States.

The book consists of a brief introduction, six chapters, a conclusion, an appendix ("Sources and Data Selection"), a bibliography of over five hundred fifty references, and an index. The chapter titles ("The Business Enterprise and the Intercorporate Network," "Network Structure and the Concentration of Capital," "Regional and Sectoral Structuring," "Family, Kinship and Corporation," "Financial Capital and the Finance Capitalists and Bank: Problems of Influence and Power") provide a brief prècis of the work.

Scott and Griff find that the regionally based boards of directors' decision-making structures that existed at the end of the nineteenth century have been slowly integrated into a national structure under the aegis of finance capital centered in London. The authors make clear, however, that the increasing dominance of London-based multiple directorships in the corporate decision-making structures seems to have been mainly an unconscious development rather than a conspiracy aimed at the domination of national industry and commerce. The main thrusts behind the movement are represented as the growing desire of entrepreneurial families for portfolio diversification and the trend toward amalgamation of regional financial institutions with the London financial establishment.

Scott and Griff support these findings with network analysis. Nevertheless, their conclusion—that the domination of the national directorate network by trading banks is responsible for the decline in Britain's economic performance—is surprising. No algorithm, other than the assertive statement that banks "encouraged cautious expansion plans and low rates of investment by nonfinancials" (p. 176), is presented to support this conjection. A better-supported interpretation of the network's contribution to "Britain's economic decline" is made in the concluding section, where the authors argue that, although pervasive in its influence, the finance capital establishment was segmented into particular spheres of influence. As of 1976, no cohesive force within this new "invisible hand" could generate the necessary decisions to prompt remedial actions.

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GORDON PHILLIPS and NOEL WHITESIDE. *Casual Labour: The Unemployment Question in the Port Transport Industry, 1880–1970.* New York: Clarendon Press of Oxford University Press. 1985. Pp. ix, 324. $39.95.

In 1914 the leader of the Dockers' Union, Ben Tillett, wrote to the chancellor of the Exchequer, David Lloyd George, that "The Casual-Labour problem is as great a problem as the wage system itself. I promised Cardinal Manning twenty-six years ago that I would stick to the dockers as long as I lived, and I have been a drudge ever since . . . But there has been so little done . . . indeed I feel impotent . . . the tragedy of it all makes one almost mad. I am surprised at times that I have reason or sanity left. I feel that to climb the hills of sorrow only lifts us to the mountains of misery there is ever yet more in sight." Preserved in the Lloyd George Collection (C/8/6/1), this letter, although not quoted in the work under review here, aptly summarizes its theme. For nearly a century underemployment at the docks, and the structural imperatives that made it inevitable, presented an intractable—indeed, an insoluble—problem. From the 1880s to 1967 casualism resisted all efforts to reform it. A commission led by Lord Devlin finally recommended measures that would end its worst aspects; when the Wilson government implemented them, however, it was too late. The dock industry itself was dying.

"The central purpose" of this excellent book, according to Gordon Phillips and Noel Whiteside, is "to explain why decasualization proved so elusive an objective" (p. 4). To fulfill this aim, they trace the evolving attitudes toward casualism of government reformers, employers of dock labor, trade union officials, and, in so far as the evidence permits, the dockers themselves. The authors describe the various schemes put forward to end the system, and they offer an interpretation of the culture of dock work. Their conclusions will not satisfy labor historians who have stressed the radicalism of the British working class, the purposefulness of employers intent on subordinating their men, or the "corporative" powers of the state. Rather, Phillips and Whiteside emphasize the "solidity of institutional and occupational conservativism" (p. 5). The attitudes of government officials, employers, and trade union leaders toward casualism varied over time. But, even when they tried to reform the system, the dockers objected, because, haphazard and cruel as the system was, it nevertheless helped them resist work discipline and "deskilling."

This book is thoroughly researched and carefully written. Its conclusions are thoughtfully and persuasively presented. One could wish for more on the culture of dock work itself. It is striking that the authors include no description of the actual labor

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process. But I do not want to quibble. This book makes a significant contribution to our knowledge of the history of port labor and industrial relations in dockland.

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STEVEN TOLLIDAY and JONATHAN ZEITLIN, editors. *Shop Floor Bargaining and the State: Historical and Comparative Perspectives*. New York: Cambridge University Press. 1985. Pp. vii, 261. $34.50.

The essays included in this volume were originally papers delivered at King's College, Cambridge in 1982 and have been brought together by Steven Tolliday and Jonathan Zeitlin. Although they are focused chiefly on labor relations in Britain, a comparative perspective is achieved by two contributions dealing with trade unionism in the United States and Italy. The articles range from those based on careful archival work, such as Noel Whiteside's study of the impact of the British government's efforts to decasualize labor on the docks, to discussions based on the existing literature such as Howell Harris's stimulating examination of the effect of the New Deal on the American labor movement.

As this collection demonstrates, the approach to labor history has changed markedly over the last two decades. The selection of shop-floor bargaining as a topic indicates a shift of interest away from the earlier practice of analyzing the policies, successes, and failures of the Trades Union Congress and the large national unions. Instead an effort has been made to discover the way labor agreements, government policy, and other political and economic factors actually shape the conditions in the work place.

Attention to grass-roots organizations can alter our understanding of broad developments. By focusing on the ship-building industry, Alastair Reid, in his essay "Dilution, Trade Unionism and the First World War," argues persuasively that the conventional view of the shop steward movement as a revolutionary response to "dilution" (the use of unskilled workers where union rules required skilled workers), which engendered opposition from both trade union officials and the state, needs substantial revision. In fact dilution was little practiced by employers, who tended to hoard skilled labor to meet the extraordinary demand. Reid concludes that labor unrest concentrated on the traditional issues of wages and working conditions, that no serious revolutionary threat was present, and that, amid the complications caused by World War I, trade union officials and government arbitrators alike generally welcomed shop-floor bargaining in order to avoid disruptions in production.

By contrast, in the United States during World War II the intervention of the state through institutions developed during the New Deal years, such as the National Labor Relations Board (NLRB), had the effect of discouraging shop-floor bargaining in favor of a "responsible unionism" bound by contracts negotiated at the national level. Harris's essay dealing with this development, although conceding that the autonomy of the unions was somewhat constricted, dissents from the view of some New Left historians that the unions, by allowing themselves to be incorporated into the system, sacrificed more radical possibilities. He argues that, given the pre–New Deal weakness of the American labor movement, the gains made during the 1930s and 1940s in recognition of unions by employers and the economic advancement of the workers were well worth the surrender of some freedom of action to administrative regulation.

Harris's essay, among others in the volume, reveals the importance of bureaucratic as opposed to political decision making in setting the pattern for labor relations. Lawyers working for the NLRB almost independently established a body of administrative law governing the relations between unions and employers, despite the indifference of the Roosevelt administration. Likewise, Giovanni Contini's essay on shop-floor bargaining in Italy, while placing emphasis on the changing political context, assigns an important role to judicial decisions in strengthening the rights of workers.

Judging by these essays, no generalization is possible concerning the stance of the state toward shop-floor bargaining. In some instances the state has discouraged such bargaining; in others it pressed employers to accept the practice. Zeitlin in an ambitious essay that serves as an introduction to the volume, attempts to develop a theory of the state consistent with these findings. After a helpful critique of current Marxist, corporationist, and pluralist theories, he concludes that none of these approaches accounts satisfactorily for the instances in which the state acts not in the interest of capitalism or in response to competing pressure from sectional interests but, rather, to promote its own distinctive concerns such as the preservation of domestic order and external security. He argues, in short, that the state is potentially an autonomous entity that may in pursuing its own priorities strike bargains with subordinate groups at the expense of social elites. This is an important essay whose implications extend far beyond the matter of shop-floor bargaining.

Students of labor history will be grateful for this volume that adds to our empirical knowledge of some episodes and raises larger interpretive issues.

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ROGER GEARY. *Policing Industrial Disputes: 1893 to 1985*. New York: Cambridge University Press. 1985. Pp. vii, 171. $34.50.

Roger Geary's analysis of the changing forms and nature of the public disorder that sometimes accompanies industrial disputes is based on a highly selective group of strikes that are examined as illustrations of the policing and picketing strategies of the time. Most attention is devoted to large-scale conflicts such as the West Yorkshire miners' strike of 1893 (where the Featherstone shootings occurred) and the national miners' strike of 1984–85. The broad themes of the book are the demise of strike-related violence until the end of the 1970s and the growing sophistication of central control over policing strategies.

In the 1890s, for example, most labor violence was directed against property, and police intervention tended to be accompanied by stoning, baton charges, and the frequent mustering of army units. From 1910 to 1914, however, confrontations with blacklegs became the main occasion for industrial disorder, and the army began to be employed with greater discretion and care. Primary responsibility for the handling of confrontation was unequivocally placed on the local police forces. Police tactics changed accordingly. The government began to exert a stricter control over police responses and thus reduced the autonomy of the local magistrates; in the early 1920s the government developed a sophisticated system of intelligence and information gathering. This system seems to have been removed from its central role in government planning after the general strike of 1926. But a further intensification of government planning and a reevaluation of policing tactics began in the early 1970s, when the development of the mass and flying picket strategies disturbed the balance between police authority and picketers' rights that had grown up since 1945. By the late 1970s, however, the contest between a political economy that accepted the power of labor in society and one that prescribed the necessity of taming that power had politicized the issue of industrial peace. Thus, police tactics became more aggressive. The ritualistic pushing and shoving by which picketers made their presence felt and police contained crowds were replaced by the use of riot control tactics to overpower strikers and to protect strike breakers. The resulting escalation of violence was most clearly seen during the miners strike of 1984–85, which in public perception was dominated by scenes of disorder, even though only fifteen serious disturbances occurred between March 1984 and March 1985.

Geary's book has been written mainly from secondary works. He does not, therefore, take us into the internal world of the decision making of the Home Office or the police, perhaps because the sources remain unavailable. Nor does he provide any broad framework for understanding the ebb and flow of different strategies for coping with industrial disorder. The dominant theme is as one would expect. From the 1890s to the late 1970s industrial violence progressively declined until Margaret Thatcher came to power. Geary's explanation for the demise of industrial violence as a product of the growing constitutionalism of labor, as the result of increased media attention, and as the democratization of civil liberties points only to the symptoms, however. None of these categories are absent from the more violent environment of the 1980s. What changed was the politics of the equation. The tougher police tactics that were formulated from the mid-1970s and employed from about 1980 were at one with such developments as the tightened legal restrictions on industrial action and were integral to the perceived need to create a "freer" labor market as part of the regeneration of economic growth. The reversal of the long trend toward declining industrial disorder was coherent with the political determination to end the "forward march of labour" and restore labor to a properly subordinate place within social and industrial relations.

RICHARD PRICE
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HEDLEY SMYTH. *Property Companies and the Construction Industry in Britain*. (Cambridge Human Geography.) New York: Cambridge University Press. 1985. Pp. ix, 265. $44.50.

This book could be worse. Hedley Smyth, writing from what he describes as a "radical perspective," attempts to describe and analyze the growth and decline of British construction firms in the years since 1939. The emphasis is on the firms themselves (Laing, McAlpine, Taylor Woodrow, Costain, Wimpey) and only secondarily on the problems of land accumulation, planning permission, and capital management. In addition, the author is concerned almost entirely with heavy construction, office buildings, and factories. He also touches on the brief boom in deep-water drilling rigs—in which most of his subjects took heavy losses—but not on residential or, in the United States, "retail" development.

Most difficult, however, is what might be termed the "jargon dimension." The book begins with a fifty-page introduction to basic Marxist economics, with excursions toward Rosa Luxemburg and Michal Kalecki. He uses terms such as "surplus value" and "concentration of capital." He notes, but does not solve, the ancient problem of why employers, for whom the expropriated surplus labor value

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is the basis of their fortunes, insist on installing labor-saving machinery. The author's stated aim is to incorporate these truths into his story. This he does only to the extent of incorporating the terminology into his prose. "Ground rent," which has a perfectly understandable definition in English law, turns out to be "unearned increment"; what most economists, or even bookkeepers, would call "unit cost" is referred to as "value."

Yet after the first three chapters this book contains much useful and interesting information. Basically it traces the revival of heavy construction in Great Britain during the Second World War. The effect of the war was to produce a shake out in the industry, driving smaller and middle-sized firms into subcontracting while the big firms got even larger. Wimpey, for example, became so expert at building airfields that it would bid on contracts based on target costs plus fixed fees by foregoing the fixed fee; the company depended on completing the job under the target cost and so collecting 25 percent of the savings. No small firm could compete.

The story since the war, as Smyth sees it, has been one of continual expansion and of developing crises, focused in the overbuilding of office blocks stimulated by price inflation. The rise in rents has led developers to value buildings on the basis of capitalized future income, whereas insurance companies and pension funds, regarding bricks and mortar as a safe repository for evergrowing funds, have been willing to purchase and lease back at unrealistic prices, so keeping developers liquid and financing further expansion. For two decades, until the mid-1970s, it seemed no one could lose. A building in existence appreciated automatically; future rental value could be imputed even without tenants, as Harry Hyams's Centre Point Tower in St. Giles Circus, near the British Museum, demonstrated. All of this was stimulated even further by the "Brown ban," imposed by George Brown of Labour's Department of Economic Affairs on November 4, 1964, prohibiting office construction in central London.

The game came to an end in November 1973 with the rise in interest rates that accompanied the oil crisis and the miner's strike. Prime office rentals in London fell by 50 percent between 1973 and 1976, and a large number of fringe banks as well as development companies went bankrupt. The problem for construction companies was that many of them had themselves gone into the development business. Smyth refers to this, ominously, as "the first phase of the crisis." The second phase, he implies, will arrive within a few years when big financial reservoirs, particularly pension funds with liabilities to a rapidly aging population, attempt to realize on the shrunken assets represented by their property holdings.

For the economic historian this is a useful but disappointing book. As I have recently pointed out in another review, the rebuilding of Great Britain in the twentieth century represents a large and important story that needs to be told. But it must be recounted without jargon and ideology and in terms that will render it accessible to the nonspecialist. So far that work has not appeared.

BENTLEY BRINKERHOFF GILBERT
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PENNY SUMMERFIELD. *Women Workers in the Second World War: Production and Patriarchy in Conflict*. Dover, N.H.: Croom Helm. 1984. Pp. 214. $28.00.

In her analysis of women's war work Penny Summerfield concentrates on the development of public policy toward such work, particularly as formulated and enforced by the Ministry of Labour and the Ministry of Health. She devotes little space either to the private sector's responses to women's employment or to the attitudes and lives of individual women. Her choice of theme and topic and her concise, well-organized style allow her to sustain a powerful, though narrow, interpretation of her subject.

When war came in 1939, the British government attempted to mobilize female workers by voluntary means, much as it had done during the 1914–18 war. Since voluntarism proved to be an ineffective labor policy, the government soon adopted conscriptive measures, although those women who had children under the age of fourteen were not directly coerced. In Summerfield's view these half-measures resulted in lost opportunities to integrate women more thoroughly and equitably into the work force, to strike blows against patriarchy, and to prosecute the war more efficiently. Consequently, Summerfield suggests that World War II was less of an agent of change than has been argued by such male historians as Arthur Marwick and Gordon Wright.

Summerfield contends that the government's policies were pulled in opposite directions because of conflicting values. The government needed to expand production to pursue the war effort and therefore required the labor of women, especially of young, vigorous women who were also likely to be rearing children. Simultaneously, policy makers found it difficult to develop a thoroughly rational plan to maximize the use of women's labor because of patriarchal attitudes about women's work and family roles. Her conclusion is that these patriarchal attitudes not only survived the war but also guided wartime policies toward women. Ultimately, patriarchal values proved stronger than production needs.

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The author provides a provocative, revisionist contribution to scholarship about the condition of twentieth-century women. Her work is well-organized and the argument clearly stated. Yet I remain unconvinced. The framework of her argument—the tension created between production and patriarchy—is too narrow. Although these two value systems were no doubt of importance, her concentration on them results in a neat dichotomization (and compression) of the issues and allows her to negate the war's impact on women to a greater degree than seems justified by the evidence adduced.

NEAL A. FERGUSON
University of Nevada, Reno

BARRY JONES and MICHAEL KEATING. *Labour and the British State*. New York: Clarendon Press of Oxford University Press. 1985. Pp. vi, 214. $29.95.

Historians of the Labour party have long recognized that the Attlee governments of 1945–51 failed to fulfill the cherished hope of building a socialist commonwealth in Britain: most have accepted the formulation of Herbert Morrison's biographers that socialism in Britain thereafter became what Attlee's regime had accomplished. Little in the later governments of Harold Wilson and James Callaghan gives reason to contest such a judgment, which is endorsed by Barry Jones and Michael Keating. Nonetheless, they reassess the role of the Attlee administration to incorporate its becoming "immersed in the administration of the British state" (p. 61). Yet this absorption of party within extant state, they argue, has its roots deep in the Labour party's history, because the party had never resolved the role that the state should play when its government passed into the hands of those committed to democratic socialism. Thus, in 1924 and 1929, as well as in 1945 and down to the present day, the Labour party has chosen not to question either the nature or the neutrality of the British state, in the process exposing itself to the criticism voiced decades ago by Harold Laski that the party fallaciously assumed "the absolute validity of the form of the political state regardless of the economic character of the society it was supposed to represent" (pp. 57–58).

Labour's pursuit of political power has consistently revolved around the question whether the party should be one of principle—electoral consequences notwithstanding—or of pragmatism. In characterizing this conflict within the party, Jones and Keating posit a tripartite division among the "electoralists," the ideologues, and those who seek to use it as the political agent of the industrial wing of the labor movement. Their analysis of the interaction between these groups is a useful contribution. The thrust of the book, however, is the often-ignored relationship between the party and "the form and institutions of the United Kingdom state" (p. v), a central theme that arose from the authors' own concerns about the Labour party's policy on devolution in Scotland and Wales during the 1970s. Ironically, their treatment of the issue of devolution, which the party has yet to resolve successfully, is probably the least interesting of their several case studies. Their discussion of the triumph of the English parliamentary tradition after the 1707 Act of Union with Scotland sheds light, however, on the party's twentieth-century acceptance of the virtual congruity of parliamentary and popular sovereignty, although the party's own origins occurred outside the parliamentary tradition. Other issues that benefit from a focus on the Labour party's concept of the state are its attitudes toward the machinery of government, its dealings with the industrial state, and its views on the strife in Northern Ireland and on British membership in the Common Market. Such is a select list, the authors admit, and those who seek enlightenment, from such a perspective, on Labour's international policy, its views on the evolving management of the nationalized industries, or its civil libertarian concerns will not find it in these pages. Whether the authors have chosen their case studies well is open to question: since conflict with Thatcher's Conservative party is particularly heated in the latter questions, the degree to which these acute political controversies stem from different relations of party to state—the Tories within the parliamentary system during its development, and Labour from without after that system had matured—might well have engendered even more comprehensive observations.

Admittedly, the party's historians may well have emulated the lack of interest in questions of the state exhibited by generations of Labour party leaders, depicting criticism from the Left in political terms rather than as a deep-seated concern about the theory of sovereignty and the role of the state. Although this corrective effort of Jones and Keating is "present-minded" and responsibly partisan, historians will benefit both from the novel perspective on the party's evolution and from the informed speculation about its future: Labour, they conclude, cannot defend the state from attacks mounted against it in the 1980s unless the party—trade unionists, ideologues, and "electoralists"—forge a common understanding of "the role the state should play in terms of economic planning, administering nationalized industries, and the relationship with the trade unions" (p. 201). Labour may face a dire future, but such an undertaking would require a convergence

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of theory and practice rarely encountered in British political history.

JOHN F. NAYLOR
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JOHN COLVILLE. *The Fringes of Power: 10 Downing Street Diaries, 1939–1955*. New York: W. W. Norton. 1985. Pp. 796. $25.00.

As an extremely well connected young man at the Foreign Office, John Colville was given the remarkable opportunity to work intermittently as an assistant private secretary to three prime ministers over some sixteen crucial years. His diaries, which have been supplemented with clear explanations and footnotes, constitute an intimate account of personal, if low-level, observations on the way in which a modern prime minister actually operates.

Torn from his fellow Tory, Neville Chamberlain, and thrust into initially reluctant service under Winston Churchill, whom he distrusted, Colville presents a fascinating and balanced portrait of the great prime minister in 1940–41, 1943–45, and, more exceptionally, 1951–55. Soon won over completely by his heroic new master, Colville revealingly cites Rab Butler on Churchill as "the greatest adventurer in modern political history" and as "a half-breed American whose main support was that of inefficient but talkative people of a similar type," such as Lady Astor and Ronald Tree (p. 122). Colville's admiration for Churchill eventually came to embrace the prime minister's family with the not unusual exception of his son, Randolph.

In agreement with the prime minister on the risky intervention into Greece in 1941 against the wishes of much of the War Office, Colville also shared Churchill's loathing for Russia and cites with relish Churchill's remark that the British (Labour) ambassador there, Sir Stafford Cripps, belonged in Moscow as "a lunatic in a country of lunatics" (p. 309). Even Colville, however, noted a tendency on Churchill's part to use General Sir Archibald Wavell as a scapegoat for his own insistence on the Greek fiasco.

In the spring of 1944, after returning from brief active military service as a pilot in the Royal Air Force, Colville noted Churchill's physical exhaustion as control of the war gradually slipped out of his hands into those of the hostile Soviets and inexperienced Americans. Nevertheless, he praises Churchill for his qualities of "imagination and resolution," which were so "conspicuously lacking among other Ministers and among the Chiefs of Staff" (p. 489). The local communist attempt to seize Greece in December 1944 roused Churchill to active opposition to the growing communist threat to Eastern Europe, and he cabled the British military commander in Greece to "treat Athens as a conquered city" (p. 533). At the same time Field Marshall Sir Bernard Montgomery's "triumphant, jingoistic and exceedingly self-satisfied" attitude toward the Americans during the Ardennes counterattack evoked disapproval on Colville's and, one surmises, Churchill's part (p. 550). At Yalta the prime minister described President Roosevelt as having looked so "old and ill" as to have "lost his powers of concentration" and having degenerated into "a hopelessly incompetent chairman" of the Big Three (p. 560).

The eminently flexible Colville was next in the service of Clement Attlee, whom he also soon admired as quick, honest, and full of common sense, yet without Churchill's or Chamberlain's vanity. Churchill's return to the office in 1951 reveals Colville's limitations as a judge of foreigners in his description of Eisenhower as "a genial and dynamic mediocrity" (p. 661). The sad final days of easing an unwilling Churchill out of office in 1954 in favor of Anthony Eden are well delineated, including Churchill's characterization of Eden as "sometimes very foolish" when he opposed the Americans in Guatemala yet ignored Soviet rule in Poland (p. 694).

Colville was wise to retire when he did, before the disaster of Eden's long-awaited arrival at the top of the greasy pole.

TRUMBULL HIGGINS
City University of New York

V. TRUKHANOVSKY. *Anthony Eden*. Moscow: Progress; distributed by Imported Publications, Chicago. 1984. Pp. 351. $8.95.

The career of Anthony Eden (Lord Avon), the British prime minister who resigned in humiliating circumstances in the wake of the Suez crisis of 1956, continues to attract biographers. In Eden's lifetime William Rees-Mogg, Dennis Bardens, Randolph Churchill, Lewis Broad, Alan Campbell-Johnson, Geoffrey McDermott, and I attempted provisional studies. In 1981 David Carlton contributed a massive biography that savagely indicted every aspect of Eden's character, career, and policies. An official life is being written. What can one expect, therefore, from a biography of Eden, first published in Russian in 1983 and now translated for an English audience?

V. Trukhanovsky has written voluminously on the subject of British foreign policy, including studies of Winston Churchill and David Lloyd George. He now turns his attention to what he describes as "the first . . . Marxist book on Anthony Eden" (p. 6). The picture that emerges is not flattering, nor is it

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original. For Eden is as easy a target for a Marxist historian as for a non-Marxist one.

Trukhanovsky is most outspoken on the 1930s "appeasement" phase of Eden's career and the Suez crisis of 1956. On the subject of the former he joins hands with Western revisionist historians. He argues that Eden was never really committed to the policy of collective security. Instead, Eden emerges as an enthusiastic practitioner of appeasement, resigning in 1938 not over matters of principle but over disagreement on means. With regard to Suez Trukhanovsky is in the company of numerous critics and historians who unanimously condemn the British invasion of Egypt. Where Trukhanovsky differs is that he places both episodes within the context of Eden's attempts to halt "British imperialism's decline" (p. 117). And even that reading would bring nods of assent from many Western observers.

The Marxist framework that structures this biography forces Trukhanovsky to make long digressions to give a Soviet perspective on world events. In the process Eden often disappears from the analysis for pages on end. The reader is also subjected to much chatty information about Eden's personal life, all gleaned from previous biographies. The book contains no original research, nor is it accompanied by footnotes, index, or bibliography. Trukhanovsky's book satisfies more than just an idle curiosity but is less than required reading.

SIDNEY ASTER
University of Toronto

JOSEF FOSCHEPOTH and ROLF STEINIGER, editors. *Die britische Deutschland- und Besatzungspolitik 1945–1949.* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh, for the Deutsche Historische Institut, London. 1985. Pp. 326. DM 58.

In June 1982 the German Historical Institute in London sponsored a conference to take stock of contemporary research on the politics of the British occupation of Germany. The following October a workshop at Mühlheim, drawing many participants from the London meeting and others, posed again the desirability of publishing the results of the conferences. In the present volume, edited by Josef Foschepoth and Rolf Steiniger, the majority of the chapters are revisions of papers presented in London or Mühlheim. The stock taking was in order since the British archival sources of the subject had been accessible for about a decade. Because another assessment, edited by Claus Scharf and Hans-Jürgen Schröder, had appeared in 1979, the essays of this book have a seasoned quality.

The principal elements in the story of British policy in Germany include: Britain's desire to reduce the prospect of a future German threat, to add to British strength by acquiring a leading influence in Europe, to extend the wartime cooperation with the United States to as yet uncoordinated policies on the future of Germany, to reduce the wartime mobilization that weakened postwar Britain by straining dwindling resources, and to withdraw radically from some of the commitments made in palmier days. The magnitude of Britain's economic problems had been underestimated and took on exploding proportions as Britain faced its zone in Germany—the destroyed industrial area swollen by refugees from Soviet Germany—and encountered the consequences of Soviet hostility.

Almost no serious Anglo-American study of long-range problems that might attend a new Germany had been made by the end of the war (p. 52). To put off planners who sought commitments, Churchill used injunctions about the wisdom of reserving decision until the facts and forces significant to the action should be revealed; Roosevelt was polonian against articulating plans for lands that had first to be won (p. 65). On a slightly lower level Attlee and Eden sought agreement on an organization to formulate allied policy toward Germany and resist fluctuations in public opinion and the changing power relationships of the leading states. Officials and bureaucrats made the future their work but "the future did not hold the course" predicated by the planners (pp. 45–46). Britain's problems were peculiarly tough on preconceptions: for example, forced to incur the loss of dollar credits for the sake of Germany, the chancellor of the exchequer, Hugh Dalton (who, as D. C. Watt put it, was never a friend of Germany) described his self-deprecated magnanimity as "paying reparations to Germany" (p. 23).

If the outlines of this story are roughly familiar, the articles, nevertheless, are fresh and sharp, rewarding in detail and in sweep. D. C. Watt is characteristically thorough and forthright in dealing with Britain, the United States, and Germany. In the first section he is lead-off man for four lively and serious authors concerned with general political subjects. Three articles grouped under the title "Ideology or Pragmatism: British Socializations Policy in the Rhine and Ruhr" conclude with one entitled "Socialization Does Not take Place." The following three chapters deal with breakdowns or failures of land reform in the shadow of hunger, of a new order of civil service (a complex story but a breakdown), and of a "democratic reform" of German universities. There is no index but there is a bibliography and a classified listing of the documents in the Public Records Office at Kew on the British Occupation Zone.

Four essays on economics and finance have been added to the material from the initial sessions. The

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final one by Ian Turner of Manchester is on the Volkswagen enterprise, a success story of the British occupation. As the chairman of its board of directors said in 1982: "It was the most successful auto business the British ever founded." The British author notes the historical irony that a comparable success did not attend British auto-makers at home (p. 299).

M. A. FITZSIMONS
University of Notre Dame

RITCHIE OVENDALE. *The English-Speaking Alliance: Britain, the United States, the Dominions, and the Cold War, 1945–1951*. Boston: George Allen and Unwin. 1985. Pp. 309. $27.00.

Ritchie Ovendale has given us a clearly written and thoroughly documented survey of British foreign policy in the first six years of the cold war. His thesis is quite straightforward. A declining Britain seized on the strength of the United States and manipulated a naive superpower to maintain "the illusion of great power status" after 1945 (p. 288). For Ovendale, Ernest Bevin is the key actor. Sharing Winston Churchill's wartime fear of future Soviet aggression, the Socialist foreign secretary drew on Neville Chamberlain's prewar blueprint to base Britain's defense on a loose alignment with the old "white" Dominions and the powerful United States. Bevin's genius, according to the author, was to exploit American strength while attempting, as he explained in 1951, to "exert sufficient control over the policy of the well-intentioned but inexperienced colossus on whose co-operation our safety depends" (p. 226).

This policy worked best in Europe, where Britain played an important role in developing the containment policy embodied in the Marshall plan and the formation of the North Atlantic Treaty Organization. But elsewhere Bevin faced a much more difficult task in trying to guide American foreign policy. As Ovendale makes clear, President Harry Truman, responding to domestic pressures, pursued an independent policy on Palestine that ran directly counter to Britain's attempt to woo the Arab states. And in Asia both the American refusal to recognize the new Communist government in China and American insistence on an aggressive strategy in the Korean War placed great stress on the English-speaking alliance. In the long run, far more dependent on the United States than they had realized, the British were forced to go along with what they believed were ill-considered policies in the Middle East and Asia.

Ovendale's account, written primarily from British Foreign Office records but supplemented by occasional references to American and Commonwealth sources, suffers from its limited focus. Presenting the early development of the cold war through British eyes, and primarily those of Bevin, the author tends to exaggerate the British role and its impact on American foreign policy. The main factor behind the evolution of containment was not Bevin and British efforts to influence the United States but rather the stark nature of the Soviet threat as perceived in 1947 by President Truman, George Marshall, Dean Acheson, and George Kennan. Nothing in this account points to any single occasion when a British action played a decisive role in shaping American foreign policy. It is thus difficult not to conclude that Bevin and his colleagues, as Ovendale admits, "had no choice in the long run but to rely on the friendship and support of the United States" (p. 225). They occasionally were able to gain slight modifications in American foreign policy, but by 1951 it was clear that Britain, along with the Dominions, had become a junior partner in the English-speaking alliance.

ROBERT A. DIVINE
University of Texas

KERBY A. MILLER. *Emigrants and Exiles: Ireland and the Irish Exodus to North America*. New York: Oxford University Press. 1985. Pp. xii, 684. $35.00.

Kerby A. Miller examines the Old and New World experiences of the close to 7 million pre-1922 Irish emigrants to America. His main concern is with the Catholic majority that arrived after 1820. Miller's thesis is that they were cultural as well as geographic aliens. Cultural exile began in their own country when commercialization associated with Anglicization began to intrude on Gaelic and Catholic communalism, collectivism, fatalism, dependency, and passivity. The majority of emigrants before the Famine of 1845 left the most commercialized and Anglicized parts of Ireland. The "great hunger" then created refugees from all over the country. After the famine determination to curb population growth in order to preserve subsistence size family farms and a continued swing from tillage to low employment grazing agriculture institutionalized emigration as an economic and social safety valve.

Catholics and nationalists adopted ambiguous stances on emigration. Deaths and departures as a result of the famine eliminated much of the rural rabble that was an obstacle to Catholic reform and devotionalism. Churchmen after the famine took pride in an expanding Irish spiritual empire. They worried, however, that urban America threatened Irish spirituality. Nationalists pointed to emigrant successes as evidence that the British presence retarded Ireland's development. But they admitted their dependence on the Irish abroad for liberation.

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Catholics and nationalists refused to acknowledge the Irish dimension of the economic and social situations that forced emigration, to advocate radical changes that might persuade people to remain at home, or to admit that during the famine family and communal relations often dissolved in selfish struggles for survival. Thus, both Catholics and nationalists masked reality in the exile theme and blamed the British as the sole cause of the diaspora.

Miller does not interpret Irish-America as a process of continual assimilation or its nationalism as primarily a search for respectability. Irish immigrants, particularly from the Gaelic West, had difficulty coping with Anglo-Protestant America's individualism and competitiveness. Irish Americans did not attain economic and social mobility or political power until the late nineteenth century. Before then, life for many was failure exhibited in alcoholism, crime, and neuroses. Serving as a transoceanic bridge of familiarity and comfort, Catholicism delivered somewhat contradictory results. By emphasizing moral order and respectability, it encouraged mobility; by stressing Catholic uniqueness, it fostered isolation. By giving Ireland's freedom priority, important leaders of Irish nationalism also rejected assimilation. Most of their followers represented the frustrations and failures of exile, manifested in hatred of the perceived British source of their plight.

Comprehensively synthesizing and analyzing the best in historiography, this book is exceptionally well written and informative. Its rejection of nineteenth-century modernization of Ireland and assimilation of Irish-American historical interpretations will stimulate healthy controversy in Irish studies circles. Miller contributes to an understanding of the distinctions in personality and psychology between Irish Catholics and Anglo-Protestants on both sides of the Atlantic, differences that have survived longer than many observers of Irish-America care to admit.

Despite its attractive style and substantial content, the study is too long and repetitive in theme. Miller did not have to present such a detailed examination of all facets of Irish history to establish his thesis about exile. A shorter, more pointed study would have had more impact.

Length is only one problem with Miller's book. Another is that it presents unbalanced "transatlantic history." The perception of Ireland is clearer and more insightful than the vision of Irish-America. Perhaps Miller's main source—emigrant letters—and his dominant interest—nationalism—distort his view of the American Irish. Many more of them were involved in American-centered Catholicism and politics than in salvation of the Old World. Irish-American adjustments cannot be measured by letters to Ireland. The unhappy were more likely to write than were the satisfied. Envelopes containing passage tickets or fares countered messages of misery.

Miller does not offer a clear or comparative standard of alienation. Were not all newcomers to urban America, including rural Anglo-Americans, cultural strangers? Just how unusual were the Irish? We do know that their adaptation to the American situation lasted for a shorter period than that of most other groups. They quickly became citizens and participants in politics and popular recreations. Relatively few considered a return voyage.

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LEÓN Ó BROIN. *Protestant Nationalists in Revolutionary Ireland: The Stopford Connection*. Totowa, N.J.: Barnes and Noble or Gill and Macmillan, New York. 1985. Pp. 234. $26.50.

This work recounts the interlocked careers of several members of that small minority within a minority, the Protestant nationalists in early twentieth-century Ireland. León Ó Broin places at the center of this little circle Dorothy Stopford, niece of the redoubtable Alice Stopford Green and herself a medical student in Dublin in the years immediately following the Easter Rising. The papers of Stopford and of her relatives constitute the core of Ó Broin's primary research. He adds to these materials a good deal of anecdotal information on other figures ranging from Douglas Hyde and William Butler Yeats to Roger Casement and Bulmer Hobson.

This work of simple description and narrative contains little analysis: it is a well-executed literary achievement and popular narrative history in which the author has made a coherent story out of episodes that are mainly on the margins of major political events. The intended audience seems to be Irish readers who are already conversant with the events and personalities of the period. I am not quite sure, for example, how a non-Irish, nonspecialist reader would understand that Eoin MacNeill, who figures prominently throughout the book, was not a Protestant but a very devout Catholic.

The reader who turns to this book to learn why a few Protestants in southern Ireland deserted their caste and supported the rebels will find no clearly stated answer. Moreover, the author fails to substantiate his claim that although Protestant nationalists were few in number, "their influence on the development of Irish nationalism was very considerable" (p. 1). It would be more accurate to say that any Protestant who was alienated from his or her own culture, for whatever reason, was welcome to lend a

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hand in the creation of what became a Catholic confessional state.

Dorothy Stopford's last active years were devoted to a campaign to control the spread of tuberculosis in the 1940s. This was the zenith of Catholic episcopal intervention in public policy, particularly health policy, and Ó Broin is at pains to minimize Stopford's frustration in this situation. It is surely misleading the non-Irish reader to introduce Stopford's friend Noel Browne as the minister of health who successfully addressed the problem of tuberculosis without even mentioning that about a year later, in 1951, his political career was wrecked in the most significant controversy between the church and the state in the history of independent Ireland.

Despite its deficiencies in analyzing the topic stated in the title, this book may be a useful, if inadvertent, contribution to women's history. Stopford's career, and her personal and family life, are sufficiently documented here for profitable comparison with other professional women in other twentieth-century societies.

DAVID W. MILLER
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PAUL CANNING. *British Policy towards Ireland: 1921–1941*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xvi, 344. $29.95.

Having read the preface and then the main body of this book, one might suppose them to have been written by two distinct authors. Whereas one is characterized by wild and illogical opinions, the other has been almost invariably fueled by steadily maintained reason. Why, if (as alleged) the United Kingdom had been defeated by the Irish guerrillas in "the troubles" preceding the settlement of 1920–21, was London able to dictate the nature of the treaty? Why was the republic not established? Why was Ireland partitioned? A comparison between London over Ireland and Washington over Vietnam would in any case be far-fetched, but, as made here, the comparison is clumsy in the extreme. The British, of course, have not searched their souls, as have the Americans after their Southeast Asian frustration, because the British knew their opponents were desperate to end hostilities. For, after all, the nigh Hookeresque shape of things heralded in the agreement between the Irish and the United Kingdom was deeply disliked by a large majority of Irish from both ethnic groups. Home rule in Northern Ireland was anathema to the overwhelming proportion of the British Irish who lived there. Dominion status was regarded as either too much or too little by the population in the south. Within the London-imposed straitjacket, however, northern Unionists rapidly came to relish a local parliament, southern Unionists gave fealty to the new Irish Free State Dominion, and a majority of Irish nationalists backed the Free State to the extent of fighting to keep it in place.

That said, it is a pleasure to proceed toward praise of Paul Canning's overall discussion of Anglo-Irish relations in these two decades. Despite an absence of material on the internal affairs of the Irish state (be it the Irish Free State or the Eire form) sufficient to explain the approach and argument adopted by Dublin toward London, the various stages of diplomatic interchange are tackled with considerable distinction. Not least, Canning skillfully handles the key political personalities who conducted foreign affairs. Neville Chamberlain may not, as claimed, have begun his politics as a Liberal, but he certainly did prove to be, as claimed, the most influential figure in this and many other scenarios throughout these two decades. Eamon De Valera, his major rival, is portrayed extraordinarily well. Indeed, the sections detailing their interchanges must be counted among the best currently in print about twentieth-century Anglo-Irish affairs. Few really appreciate the amazing degree of incomprehension about Ireland and Irish matters among the British peacemaking leaders of the early 1920s. In this study the whole awful truth of it pours forth, beautifully arranged and cumulatively compelling in its message. The exact nature of Dublin's neutrality in World War II has always been difficult to grasp. With the information and interpretation given here, this no longer need be the case. Warts and all, this is a useful and necessary work.

MICHAEL HURST
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COLETTE BEAUNE. *Naissance de la nation France*. (Bibliothèque des Histoires.) Paris: Gallimard. 1985. Pp. 431. 145 fr.

Nations, as everybody knows, do not come ready-made. Societies develop and grow into nations, and, in most cases, nations are purposefully put together by conquest and propaganda campaigns that create a national consciousness.

The present book is a beautifully written and eminently readable history of the mentalities that created the French nation. The chapters are divided into three parts. The first deals with the legends created by historians, the second with the contributions of theologians, and the third with signs and symbols. These divisions seem arbitrary. What the reader finds are nine chapters devoted to the myths of Trojan descent, the patron saints, the saintly

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kings, Salian law, the lilies, and language and culture. It is a pity that the story breaks off before the French revolution, for the great revolution succeeded where the saints, royal or not, and the church had failed—that is, it succeeded in grafting a modern political ideology on the rich crop of religious allusions that had been created during preceding centuries. One might even argue that, without the retrospective streamlining done by the revolution's modern ideology, Colette Beaune's story would have been more fragmented, less unifying, as well as less satisfying.

One might express the criticism that the author, though critical when it comes to details, is insufficiently critical of the larger issues. Thus, for example, she begins chapter 10 by saying that most medieval nations were multilingual, as if she wanted to create the impression that nations predate unitary language habits and were social realities long before the suitable mentalities had emerged to create a national reality. She seems unaware that nationhood depends in most cases on crowding out other languages as well as on subjugating those ethnic groups that cannot resign themselves to voluntary absorption. Admittedly, the plan of the book absolves the author from considering the real pressures that forged nationhood. Devoted as it is to mentalities, this study contains no chapters on Philip Augustus, Philip the Fair, or Louis XIV, let alone on Saint Joan and Saint Remi.

Even allowing for the self-imposed limitation, the book would have gained if the author had taken a more detached view of her subject. Modern historians should write not to celebrate but to enlighten. As it is, the reader gains the impression that Beaune's book is itself part of the subject matter. France, after all, is not unique, and national consciousnesses were created in other parts of the world and in other ages with the help of similar subterfuges. The great paradigm of the author's story is the biblical history of the children of Israel, who also used legends, leaders, festivals, and so on (as well as the idea of having been chosen by God) to create the national consciousness around which the so-called nation eventually took shape. Again, it would have been salutary to distinguish between the use of the *lys*, Salian law, Troian descent, and specifically New Testament Christian habits of thought and institutions. Although lilies and laws are fair game, the employment of specifically Christian symbols required a mutilation of their meaning. Jehovah, as seen by the ancient Hebrews, might well have been the kind of God who picked the children of Israel to promote his designs, but the God of the New Testament is supposed to be even-handed and could not possibly be thought to have chosen any one nation as his favorite, not even so nice a nation as the French! Beaune writes with such loving care and devoted scholarship that it is by no means clear whether she is aware of this or not.

PETER MUNZ
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MARIANNE CALMANN. *The Carrière of Carpentras.* (Littman Library of Jewish Civilization.) New York: Oxford University Press. 1984. Pp. 286. $29.95.

By the end of the fourteenth century Jews had been expelled from most regions of France. One of the exceptions was the northern Provençal area of Avignon and the Comtat Venaissin. Ruled by the popes from the late thirteenth century to 1791, Avignon and the Comtat provided an isolated refuge for the Jews. In this circumscribed domain a number of municipalities housed Jewish communities. After 1624, however, Jews were restricted to towns that hosted a ghetto and a synagogue, meaning Avignon, Carpentras, Cavaillon, and l'Isle-sur-Sorgue. Marianne Calmann's study focuses on Carpentras.

Given the paucity of studies of individual late medieval and early modern Jewish communities, the decision to focus on one such community, its history, and the various facets of its existence is a welcome one. The author indicates a number of factors that influenced her choice of the Jewry of Carpentras as the subject of her study. These include the availability of considerable primary source material and of a body of secondary literature, little of it in English. She has shaped this material into a coherent and rounded portrait of Jewish life in Carpentras. Whereas the first and last chapters treat the origins of the Jewish community of Carpentras and the fate of this Jewish community during the French revolution and after, the intervening eight chapters afford a broad overview of aspects of Jewish life in the city. Chapter headings include: "People and Buildings," "Everyday Life," "A State within a State," "The Finances of the Community," "The Economic History of the Carrière," "Language, Literature, and Learning," "The Church and Conversion," and "Relations between Jews and Christians." The result is a satisfying overall portrait of an interesting minor enclave of Jewish life.

The strength of the book—its broad and inclusive treatment of the Jewish community of Carpentras over a period of more than five centuries—is also its weakness. Inevitably the treatment of the various issues touched on by Calmann cannot be exhaustive. To take but one example, the chapter on the church and conversion treats a topic of general interest, which has generated a considerable scholarly literature. The issues associated with conversion of the Jews during the late Middle Ages and the extensive

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literature that has developed are not fully reflected in the text or in the notes. References are generally limited to the literature on Carpentras and the Comtat. Those looking for an intensive and comparative analysis of key issues in late medieval Jewish life will be disappointed. Again, however, this deficiency results from a decision on the part of the author to present a broad and rounded portrait of the Jewish community of Carpentras. This she has done.

ROBERT CHAZAN
Queens College,
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PHILIPPE LOUPÈS. *Chapitres et chanoines de Guyenne aux XVII^e^ et XVIII^e^ siècles.* (Civilisations et Societés, number 70.) Paris: L'Ecole des Hautes Etudes en Sciences Sociales. 1985. Pp. 590. 420 fr.

Any historian who has traveled to the vineyards of Bordeaux and the rich river valley of the Garonne may have wondered about the once-extensive ecclesiastical establishment of Guyenne. Philippe Loupès carefully examines one aspect of it: the cathedral and collegial chapters of canons. In this study he provides a detailed account of the numbers and functions of canons, those generally more comfortable clergymen who kept a keen eye on their rights and revenues. Tables and maps show the diversity of the chapters of Guyenne. Although not as numerous, large, or rich as the great chapters of northern France, they included urban and rural communities, each with certain dignities and claims. In contrast to abbeys and priories, chapters preserved their medieval patrimony, surprisingly not much affected by the waves of forced land sales in the sixteenth century; their properties did not increase appreciably in the seventeenth and eighteenth centuries, when pious donors gave more to new congregations and the chapters themselves were unable to buy land. Chapters did hold collections of fiefs as seigneuries and in many parishes levied tithes, their main sources of income. Thanks to a building boom in Bordeaux and to better management, seigneurial revenues increased markedly in the late eighteenth century. Like secular lords, canons complained about the burden of charges—costs of services, maintenance and improvement of buildings, legal fees, debts, and taxes. These charges had to be met from revenues that fluctuated with land sales and grain and wine prices; to balance their budgets, canons tried to cut expenses, raise extraordinary income, and borrow money. Chapter revenues generally tended to rise nominally but remain stagnant in real terms. Meanwhile, most canons enjoyed personal fortunes that placed them on the level of average Bordeaux merchants.

Chapters were not only economic institutions but also social communities with internal and external dependents, so typical of the *ancien régime*. The canons wanted a harmonious, orderly world, but their situation practically guaranteed disputes about rank and rights. Social origins of canons in Guyenne varied: nobles, who often held chapter offices, were in the minority; "hommes de loi" predominated. Humbler types were also represented, although peasants and merchants were rare. Education, somewhat limited earlier, became uniform by 1680, when nearly all canons had studied at both seminary and university. Loupès rightly emphasizes the solidarity of canons and their families, which tried to keep these desirable posts over several generations.

While Loupès brings to light useful comparative data for social and economic historians, he never loses sight of the canons' religious obligations. Divine office, chapter meetings, and administration receive their due. Of course, attendance at meetings cannot be exactly correlated with the weather (p. 279) but does reflect societal conditions as well as individual factor. That Loupès has considered so many possibilities indicates his thoroughness. His subjects may well have enjoyed "la meilleure vie du monde," at least until the long campaign against their privileges culminated in 1790. Loupès writes in a consistently respectful style, with bursts of spirit: although a work of this type cannot be called light reading, the determined student will find many rewarding insights. We should be grateful that our French colleagues are still producing work of this high quality.

MAARTEN ULTEE
University of Alabama

JOSEPH BERGIN. *Cardinal Richelieu: Power and the Pursuit of Wealth.* New Haven: Yale University Press. 1985. Pp. ix, 341. $30.00.

Joseph Bergin confides in the preface that his book was the result of an accidental discovery of five previously overlooked volumes of Paris notarial acts relating to Richelieu's financial affairs between 1631 and 1642, a fortunate discovery that has led to a brilliant, fascinating, and elegantly written book. Bergin's study of Richelieu's fortune is in the tradition of similar studies of the fortunes of well-known political figures of the Old Regime by Jean-Pierre Labatut, Daniel Roche, and François Mougel, but it is the first book-length study. The author's initial plan was to write a short article on Richelieu's revenues. But Bergin soon realized that he needed other nonnotarial sources in order to do anything with the documents he had found; he has thus

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relied heavily on Pierre Grillon's edition of Richelieu's papers, which is still appearing, and he has done extensive secondary reading. As he writes, "Slowly, the whole project grew beyond anything that I would have thought possible at the beginning" (p. viii).

Separate chapters explore the various components of Richelieu's wealth—family inheritance, offices, royal domain, *rentes*, and church benefices. The cardinal's father died a bankrupt, but his older brother Henri had succeeded in repurchasing most of their father's estates by the time of his early, violent death in 1619. The total value of these lands was in excess of 200,000 livres, a modest enough fortune, when Richelieu became the king's chief minister in 1624. When he died in 1642, he left the staggering sum of 20,000,000 livres, a revision by Bergin of the traditional figure of 22,400,000 livres, which was roughly half of the budget of the government at the time. The book's value is its use of a mass of new evidence to show the process by which Richelieu built up this huge fortune.

Revenues from the marine office of *grand maître* varied between 200,000 and 240,000 livres a year and by the late 1630s represented the second largest net contribution to his budget. These revenues were a major source of the cardinal's wealth and surprisingly were more important than the eagerly acquired lands in his native region of Anjou-Poitou and his massive purchases of royal domain land. More important than his offices, estates, royal domain lands, and *rentes* or public financial investments, however, were his ecclesiastical revenues or church benefices. Georges d'Avenel recognized their importance a century ago but exaggerated their value, which Bergin sets at 55,000 to 60,000 livres a year in 1624 and over 310,000 livres per annum by 1642. He describes in detail Richelieu's great success as a benefice-hunter. An intriguing final chapter examines the cardinal's last will and testament and the fate of his fortune after his death.

Bergin's study of Richelieu's wealth is of the same caliber as the studies of the cardinal by d'Avenel and Maximin Deloche and belongs to a recent revival in Old Regime financial history led by Daniel Dessert and Richard Bonney, whom he frequently cites. His book places the cardinal's political and financial behavior firmly within the context of aristocratic family ambition and strategy, to which Richelieu was thought to have been opposed, and shows that he shared many of the fundamental attitudes, aspirations, and modes of behavior of the contemporary French nobility, a view that has been suggested by Orest Ranum. Once in power, Richelieu imitated the *grands* in every possible respect: he acquired, for example, a territorial block of estates to provide a provincial power base. Bergin writes, "Richelieu's central conviction seems to have been that power required magnificence in order to achieve its objectives of assent and respect" (p. 6). The cardinal became the greatest and most successful noble of them all, and his success in realizing his political and financial ambitions explains some of the opposition and hostility he encountered from the great nobles who were the source of many of his acquisitions. In particular I enjoyed the book because of the way Bergin linked the accumulation of wealth to its political uses and explained its relationship to patronage and power. This is a fine book that was a pleasure to read.

SHARON KETTERING
Montgomery College

WILLIAM BEIK. *Absolutism and Society in Seventeenth-Century France: State Power and Provincial Aristocracy in Languedoc*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1985. Pp. xvii, 375.

William Beik's book is an impressive addition to our understanding of the relationship of absolutism and society. Although centered on one great province, his conclusions and method could with great profit be extended beyond Languedoc and will enliven the venerable, frequently acrimonious, and often-befuddled debate about the nature and development of absolutism in early modern France.

The subtitle of Beik's book alludes to the relationship whose disentanglement provides the key to understanding the historical development of French absolutism. The mature absolutism of Louis XIV's later years, Beik argues, lay in the cementing of a mutually satisfying alliance between provincial elites and the state that, at the time of the Fronde, may have seemed unattainable. Beik's task is to explain how this alliance was forged in Languedoc between 1620 and 1690 and to place this development in the context of an absolutism seen "not as a modern state grafted onto a premodern society, but as the political aspect of the final, highest phase of a venerable, though modified, feudal society—a society in transition, if you like, from feudalism to capitalism" (p. 339).

In his first chapter Beik identifies the two general problems around which the book is organized. The first is Louis XIV's dramatic success in ruling France more effectively than did his immediate predecessors; the second is the relationship between state and society. Succinctly, but comprehensively, Beik critiques the contribution of three major schools of interpretation: institutional, social, and Marxist. He explores the strengths and weaknesses of each school and emphasizes their lack of ability to develop synthetic explanations, primarily because

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their starting assumptions often blind each to the ways in which their interpretations can be misconstrued by the other schools. Beik distills two general conclusions from this analysis. First, the ties between society and government in seventeenth-century France were much closer than has traditionally been thought. Even at its most successful moment, absolutism was roughly congruent with social realities. Second, Beik sees absolutism as a system designed to protect, rather than threaten, class and elite interests. This extra-economic coercive system (in a neo-Marxist sense) worked to conserve the authority of the traditional landowning and officeholding classes while sharing with them the profits from the continued exploitation (primarily, though not entirely, through increased taxation) of the vast majority of nonelites.

Readers with an aversion to neo-Marxist analysis might be tempted to stop at this point. That would be a mistake. For Beik goes on to define, count, and analyze the ruling class of Languedoc in a richly informative manner. Three overlapping circles of privileged nobles, officials, and landlords dominated the province. Through their administrative functions, cliental ties, and wealth, this ruling class, under the influence of steadily developing linkages to the central government, enabled absolutism to operate in an increasingly effective manner.

How was this accomplished? Beik argues that political authority at the provincial level was above all based on the personal authority of the rulers, which derived from royal authority. Under Richelieu and Mazarin, the central government had intervened in provincial affairs in a divisive and heavy-handed manner, which undermined the personal authority of local rulers. Louis XIV developed an alliance that served the purposes of national policy by maintaining and building on the authority of local rulers (individually or collectively), thereby solidifying their class or social domination of the province while producing more orderly, efficient, and fiscally profitable governance.

This program succeeded for many reasons, not least because absolutism produced a gold mine for both king and local rulers. Of the revenues from greatly increased taxes and fiscal operations, "roughly half of it stayed in the province, and a third was channeled directly to the notable rather than the king. The cost of all these activities was then passed on to the province" (p. 332). In addition, the quartering and movement of troops was increasingly regulated, manufactures and public works introduced, and the Huguenots repressed. As Beik concludes, "Louis XIV's success in Languedoc stemmed from his almost instinctive ability to reinforce class rule. . . . But the reason his policies worked so well was that they took place in an atmosphere of social confidence. Under Louis XIV it was perfectly clear that hierarchy would be reinforced, the claim of the privileged to their share of society's resources would be guaranteed, and collaboration would be properly rewarded. Languedocian society was shored up, not levelled" (pp. 333–34).

This summary of Beik's thesis fails, however, to convey adequately the depth of research, insight, and historical skill evident in the book. In my opinion it is one of the outstanding works on early modern France produced in the last decade. Its analysis of absolutism as the highest form of feudalism, to coin a phrase, will be controversial. But it solved to my satisfaction many of the contradictions that have plagued research into the nature of absolutism. Its explanation of the development of absolutism as a system of social or class, as well as political, power is convincing and goes a long way toward solving the sometimes seemingly paradoxical successes of Louis XIV's mature absolutist system. It will be a most influential work, one with which all scholars of the period should be familiar.

JAMES B. WOOD
Williams College

DALE K. VAN KLEY. *The Damiens Affair and the Unraveling of the* Ancien Régime, *1750–1770*. Princeton: Princeton University Press. 1984. Pp. xii, 373. $35.00.

At first glance, Robert-François Damiens's attempt to assassinate Louis XV in January 1757 would seem to have had little to do with "the unraveling of the *ancien régime*." Dale K. Van Kley not only succeeds in establishing a firm link but also goes on to find origins of revolutionary ideology in the Damiens affair and the political and religious struggles of mid-eighteenth-century France. Damiens's attack on the king provides the author with the springboard for a leap into the issues dividing parlementaires, Jansenists, and other opponents of crown policies from supporters of royal absolutism. Even more spectacularly, the author dives from Damiens down to 1789 and beyond as he discovers the seeds of postrevolutionary politics in mid-century events.

Van Kley disarmingly reports in the preface that he nearly cut his research loose from Damiens to concentrate on the theoretical issues. In my opinion he was wise not to do so. His dual treatment, although it makes for a less than fully integrated book, allows him firmly to anchor the abstractions of the constitutional debates and the refusal of sacraments controversy in the social reality of an attempted assassination. The author has unearthed valuable material on Damiens's background and his exposure to the sedition-inciting anticrown rhetoric current in aristocratic households, material that helps explain how a ne'er-do-well itinerant domestic

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servant turned into a would-be assassin. A chapter on other instances of popular sedition places the Damiens episode in a broader social context and represents excellent use of police records. These chapters are almost unique in the literature on French eighteenth-century politics in their convincing linkages of elite concerns and popular expression.

Even more prodigious research has gone into the chapters on the controversy about the refusal of sacraments and on the constitutional debates over royal authority. Although the author cannot establish a fully persuasive case for placing the origins of the French revolution in the disputes involving Jansenists and Parlement-based opposition to the crown in Louis XV's later years, he shows that there was a stream of protorevolutionary ideas and action that owed little or nothing to the Enlightenment philosophes. This stream flowed naturally from the Old Regime's longstanding religious and political divisions. In fact, the evidence for the evolution of political breakdown from *ancien régime* divisions is if anything even stronger than Van Kley argues. Had he chosen to consider previous assassinations and assassination attempts, earlier antiroyal constitutional arguments, and prior episodes of scurrilous, seditious expressions of popular opposition, he would have been able to place the debates of 1750–70 in a continuum stretching back to the seventeenth and even the late sixteenth centuries. Typically, specialists in the last century of the Old Regime do not seek a context in earlier periods of French constitutional struggle, and, unfortunately, Van Kley is no exception. This absence of such a context is a loss to all concerned. Thus, it is odd to find that in the final paragraph of this book Damiens's trial is described as an "anachronistic, history-bookish staging" (p. 270). The author's detailed study shows that this was an event deeply embedded in the culture of the Old Regime, although Van Kley, himself running the risk of anachronism, would seem to want to remove it from its natural setting and see the Damiens affair only as a preface to the era of revolution.

In all other substantive respects, however, this book is an important step forward. One eagerly awaits more work from its author's pen; the anticipation would be all the greater if Van Kley were to adopt a writing style a bit less taxing of the reader's patience for rococo sentence structure.

JOSEPH KLAITS
Oakland University

THOMAS E. CROW. *Painters and Public Life in Eighteenth-Century Paris*. New Haven: Yale University Press. 1985. Pp. 290. $40.00.

The subject of this book is the formation of a public response to painting in the context of the salon exhibitions in eighteenth-century Paris. Thomas E. Crow examines the impact of the state, private patrons, the Academy of Painting, critics, the theater, and the fairs on the development of a public that by the 1780s associated artistic values with political liberalism. He asks whether the salon crowd was more than a heterogeneous collection of people experiencing art in the cramped, noisy Salon Carré of the Louvre. One of the author's principal concerns is to determine who represented the attitudes of this emerging public and, consequently, who defined the public.

Beginning with La Font de Saint-Yenne, salon critics were self-proclaimed spokesmen of public taste. La Font attacked the trivial art of François Boucher and Edme Bouchardon displayed in the salons in favor of the sober classical style of Nicolas Poussin. The artists of the academy feared and despised the critics for threatening to undermine their lucrative production of rococo decoration. As a result, the academy tried to protect itself by defining a rigid hierarchy of genres that reflected the social division between *noblesse* and *roture*, while a body of public opinion grew that was out of touch with salon art and that foreshadowed the foundation of a new liberal social order.

Several individuals and events are significant for Crow in the making of a popular artistic culture. The festive life of the fairs paralleled the public space developing in the visual arts, and Antoine Watteau juxtaposed the Harlequins and Colombines of the fair theater with refined courtiers. The author also stresses the division that developed within the salon over what constituted *noblesse* in painting. On one side were the groups that pressed for a larger role for the public in the determination of artistic worth: the critic Louis Petit de Bachaumont, Madame de Pompadour and the Lenormand clan, and the circle of Pierre Crozat, for whom the *noble homme* was a propertied bourgeois. On the other side was the court aristocracy, including financiers, which represented a contrary view of nobility defined both socially and artistically. The clash between the two groups led to an era of passionate debate over art. Crow finds charged political meanings in the criticism of the 1750s, and he links it to the struggle between the Parlement and the crown over fiscal and religious issues.

The author concludes by examining the radicalization of critical opinion on the salons of the 1770s and 1780s. The critics stressed the inseparability of politics and art and the accountability of the academy to an informed public. Jacques-Louis David emerged as the foremost critic of the academy and the state. The critics hostile to both, such as Antoine-Joseph Gorsas, a future Girondin, praised David's

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Horatii for defying the academy's rules. Gorsas, Jean-Paul Marat, and others spoke for a defiant, discontented public and created the symbols of prerevolutionary radicalism echoed in the coded political meanings in David's art.

Throughout his book Crow insists on distinguishing between an audience—a group of people who can be defined by occupation, sex, age, and other factors—and a public, which is a commonality with "a shape and a will" (p. 5). This is the central problem with the book. Crow's public is elusive, amorphous, and transitory. Its role is to evaluate art in a democratic setting, but the reader never knows who comprises the public or what its taste really was. Indeed, Crow constantly emphasizes the ideological instability and social diversity of the public and of salon criticism. In the course of the century the critics defined various publics that were abstract and fictionalized. The author tells us that the public was not a stable entity, yet his book purports to trace the emergence of a definable public ready to challenge the Old Regime and its culture. This is really not a book about the salon public but one concerned with the various groups that claimed to speak for it. The book is especially rich in establishing imaginative links between art and artistic opinion and the social, political, and intellectual developments of the age.

ROBERT M. ISHERWOOD
Vanderbilt University

DOROTHY VEINUS HAGAN. *Félicien David, 1810–1876: A Composer and a Cause*. Syracuse: Syracuse University Press. 1985. Pp. xiii, 239. $32.00.

Of all Clio's children, musicology has proved to be the least docile member of her brood. Because many musicologists are often at heart performers or composers, they sometimes picture their scholarship as essentially ancillary to the profession of music. Professional musicians are trained to cooperate in the accurate representation of scores. Analyses that stress the social and political context of music may seem mere speculation, if not an intrusion of opportunistic political and economic criteria into the chaste cosmos of art. Professional historians, on the other hand, are often ignorant of music. A glance into the typical textbook of Western civilization reveals that many historians who know how to use the visual arts and belles-lettres as evidence have no idea of the significance of music.

This very elusiveness of music makes it all the more attractive as a contemporary historical frontier. At present, many historians are investigating music. By avoiding the crowds of researchers plying obvious topics such as wars, nation-states, revolutions, and economic changes, interdisciplinary researchers are collaborating in the reunification of musicology and history, thus following in the paths of Theodor Adorno, Romano Guardini, Leo Schrade, Paul Henry Lang, and Jacques Barzun. Major historical conferences today normally include sessions that treat music. Ralph Locke, Rey Longyear, Marion Miller, David Large, David Loromer, Jeremy Mattson, James Leith, Carl Dahlhaus, Henry Kmen, and Joseph Kerman, among others, enjoy the luxury of a field that is both relatively unexplored and very important.

One could scarcely find a better subject for the study of music as a document of the history of ideas than Félicien David, for this composer's life and work summarize the special cultural features of his time. Since his compositions are not of the first rank, historians can comfortably by-pass the meticulous theoretical dissection that tends to isolate the greatest works of art as universes unto themselves. David's life and music are best presented as documents of the age of democratic revolution.

Dorothy Veinus Hagan's book establishes connections among topics that are too often studied separately. Socialism, revolutionary movements, romanticism, church liturgy, mass propaganda, education, anthropology, philosophy, and, musicology blend naturally into a coherent whole. The circle of David's associates is a glossary of the leading artists and thinkers of early nineteenth-century France. His attempt to translate utopian doctrine into music is a compact parable illustrating an ideology formulated by Plato and since followed by every major institution in Western civilization.

The book is brief and written in nontechnical language. The introduction by Michael Miller is a concise essay that would do honor to a magisterial teacher of undergraduates. As such, it is an apt beginning to the biography of a practicing artist who amalgamated the sublime and the popular.

CONRAD L. DONAKOWSKI
Michigan State University

MARTINE SEGALEN. *Love and Power in the Peasant Family: Rural France in the Nineteenth Century*. Translated by SARAH MATTHEWS. Chicago: University of Chicago Press. 1983. Pp. viii, 206. $21.00.

In this book Martine Segalen explores whether nineteenth-century French peasant wives were as subordinate to their husbands as the ethnographers who observed them and the recent historians who have relied on the ethnographers' opinions have believed. Like most historians of women and of the family, Segalen argues that the ethnographers' views were distorted by bourgeois values and that, contrary to the picture of female subservience and male callousness painted by them, the relationship between peasant husbands and wives was character-

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ized by mutual dependence and cooperation. Her argument is based primarily on an analysis of the rural proverbs collected by folklorists and of the folklorists' observations of peasant life without their judgments. Indeed, peasant wives were far from subservient and powerless: Segalen argues that the proverbs reveal a world in which husbands and wives struggled with each other for power and in which women's responsibilities for the care of the family and their relationships with other women and to the church gave them power that men often feared and that the community sought to restrain through the ritualized humiliation of the weak husband and the strong wife. It is ironic, in this regard, that the translator has rendered the central phrase of the book, "mari et femme," not as "husband and wife" but as "man and wife," a phrase that emphasizes male dominance in marriage and undercuts Segalen's major point.

In arguing for mutual dependence and respect between husband and wife and in outlining the complementarity of women's and men's work within the family, Segalen follows in the footsteps of social and demographic historians such as Owen Hufton, Joan Scott, and Louise Tilly. Overall, she presents a good case against the ethnographers and their modern followers, although historians of rural France will have some objections to the general nature of her description of rural life since agricultural work, household structures, and family life varied considerably from region to region.

The most interesting aspect of the book is Segalen's interpretation of nineteenth-century proverbs and rituals. (Fortunately, given their importance to the argument, the proverbs are included in the text in both French and English.) Her analysis is far more sophisticated than that of the nineteenth-century ethnographers. Nevertheless, in her desire to correct the folklorists' dismissal of the importance of women's economic and domestic responsibilities, Segalen herself dismisses the evidence of patriarchal values in the proverbs and rituals. Although her work is an important corrective to earlier views of women, it is too brief to develop a complex analysis of women's status in the peasant family and in peasant society. One wishes the book were longer and the complexities more fully explored.

GAY L. GULLICKSON
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NICHOLAS PAPAYANIS. *Alphonse Merrheim: The Emergence of Reformism in Revolutionary Syndicalism, 1871–1925*. (Studies in Social History, number 8.) Boston: Martinus Nijhoff; distributed by Kluwer Academic, Hingham, Mass. 1985. Pp. xx, 184.

This is a competent biography of an important figure in early twentieth-century French syndicalism. Alphonse Merrheim was the leader of the French metalworkers, building on his own family background as a copper worker. He also was an influential trade-union writer, self-taught, and less flashy than some publicists of the Confédération Générale du Travail (CGT) but thoughtful in addressing a wide range of subjects between 1900 and his collapse in 1923. Merrheim was a pragmatist by nature and through his experience in actually sustaining a host of strikes and organizing a major industrial federation; he was also intellectually convinced that, although a general strike was a suitable long run goal, much must be accomplished in terms of class education before workers were seriously ready to undertake it. So Merrheim was a reformist in a movement prone, rhetorically at least, to greater radicalism.

Nicholas Papayanis offers a political narrative, taking up Merrheim when he emerges as a labor leader and dealing with his reactions to major strikes and union developments, including World War I and the postwar labor turmoil. We see Merrheim's position during the heated debates within the CGT after 1906 and his complex role as an antiwar (and often cautious) reformist after 1914. Merrheim's relations with other key figures are adequately traced, not only his recurrent battles with the more rigorous idealogues but also his more subtle interactions with Léon Jouhaux and Albert Thomas. The narrative account develops particular richness in dealing with the wartime and postwar periods. Prewar material, particularly between 1906 and 1914, too often has Merrheim reacting to familiar developments in the French labor movement; although Papayanis has ample command of the literature and the issues, he sometimes adds little that is new. An exception, however, is an excellent chapter on Merrheim's own intellectual outlook and evolution, which breaks from the narrative mold and provides a solid intellectual survey of a working trade unionist.

The book has some unfortunate omissions, although many of them are inevitable. Beyond a birth notice, we learn little of Merrheim's early life. At no point do we see much of his personality. A rigorously private man who left few records save those intended for print, Merrheim clearly escapes a more comprehensive biographical effort. Because of the focus on interactions with larger events, the book also offers relatively little on Merrheim's patterns and activities as a leader of the CGT. Merrheim was one of the most successful industrial unionists in France; although we learn about his basic accomplishment—forming the Metalworkers' Union—and certainly grasp Merrheim's impulses during specific strikes, his administrative activities might well have been explored more fully. Finally,

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Papayanis provides surprisingly little analysis or assessment of Merrheim's impact. He conveys his sympathy for his subject and offers good, brief judgments in the course of his account, but he might have offered a bit more sense of what difference Merrheim made in a labor environment that often troubled the great labor leader himself.

PETER N. STEARNS
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JUDITH F. STONE. *The Search for Social Peace: Reform Legislation in France, 1890–1914*. (SUNY Series on Modern European Social History.) Albany: State University of New York Press. 1985. Pp. xvii, 260.

The early decades of the Third French Republic were ones of a quiet revolution, not in the political superstructure but in the lives of the citizens and in the social fabric of the nation. Legislators and other public officials proposed and implemented a broad spectrum of social legislation and programs. Six conditions provided an impetus toward social reform: (1) centralized government had been established, and the continuity of powerful bureaucracies and state agencies overrode the transience of governments; (2) republican fears of socialism and antirepublican clericalism that threatened to topple the fragile republic spurred legislation for social programs; (3) active groups committed to reform (such as doctors, social hygienists, and welfare organizations) achieved a degree of power; (4) the fear of depopulation contributed to a change in attitudes and led to a redefinition of the social question so that state and local governments could take legislative and programmatic action; (5) an economic depression contributed to new working-class problems; (6) finally, working-class interests became integrated into the political process, and advocates of the concerns and perceived interests of the working classes appeared. This emerging political importance of the working class provides the basis for Judith F. Stone's detailed analysis of the political intricacies of social reform legislation during two decades.

As the starting point of major social reform, Stone focuses on the 1890s, rather than on the two earlier decades, because she gives primary importance to an economic shift of the times—the intensification of industrialization. New economic activity led to increased activism and organization of the working class, both at the workplace and in politics. Stone also posits changes in attitudes of the middle classes and in the political parties themselves. She concentrates on changes within the Radical party in the 1890s. After the Boulanger affair and Panama crisis the party had to reorganize. Radical party leaders recognized that they could address the entire working class as a group (not just the women and children who Stone believes were marginal to national social-reform policy). According to Stone, when they discussed social reform for adult men, the nation's citizens, politicians considered the working class as a whole. Social reform, to Stone, consists of labor legislation for men in the workplace. In concentrating on men, she ignores the spate of social welfare legislation for women and children that appeared during the 1870s and 1880s as well as the application to all workers of the initial proposal of 1879 to regulate working hours, although the final compromise version applied only to women and children.

The strengths of this book lie in the clear articulation of solidarist and interventionist ideologies and in the discussion of political realignments that were both a cause and a consequence of the efforts to create a reform majority. The Radicals, more than any other group, had an immediate interest in reducing class conflict, and members of the Radical party worked with socialist and conservative representatives to find legislative solutions to keep the peace among the organized, militant, and enfranchised workers. Three categories of legislative proposals emerged: insurance plans, factory regulation, and projects to improve labor relations. By 1910 the Radicals triumphed in their efforts to direct the political struggle for social reform.

Stone's book will spark a lively debate since it focuses on the period from 1890 to 1910 and ignores the political and social reforms of the two earlier decades of the Third Republic. Furthermore, although it sets forth issues, ideologies, and political action aimed toward an "amelioration of working-class conditions through law" (p. 100), the book leaves unexplored those actual economic and social conditions and does not evaluate the success or failure of social reform policies. Despite the absence of a bibliography, this volume contributes a wealth of detail to historians exploring social welfare during the Third Republic.

RACHEL G. FUCHS
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ROBERT SOUCY. *French Fascism: The First Wave, 1924–1933*. New Haven: Yale University Press. 1986. Pp. xix, 276. $25.00.

France of the 1920s saw the rise of militant nationalist leagues—the *Légions*, the *Jeunesses Patriotes*, the *Faisceau*—that emphasized action, youth, virility, and national regeneration against democratic decadence and Marxist danger. Along with older organizations such as the royalist *Action Française*, these movements constitute the first wave of French fascism whose waxing and waning Robert Soucy describes.

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He traces the origins of these organizations to the "fascist characteristics" of three nineteenth-century movements—the *Ligue des Patriotes*, the *Action Française*, and the *Ligue antisémitique*—that were antiparliamentary, anti-Semitic, anti-Marxist, anti-liberal, nationalist, militarist, and prone to violence. He attributes their brief efflorescence to the conservative backlash that followed the victory of the *Cartel des Gauches* in 1924. All "fascist" movements sought to lean on conservatives, Catholics, and nationalists chafing at the advance of the Left. The *Jeunesses Patriotes* began as a youth auxiliary of the *Ligue des Patriotes*; the *Faisceau* was a heretical offshoot of the *Action Française*. All poached on each others' preserves for members and for funds.

Soucy examines the founding, funding, leadership, and membership of these organizations as well as of the shorter-lived *légions* that they absorbed. He shows their following to have been largely middle and lower-middle class, their ideology to have been close to that of antidemocratic conservatives, and their programs to have defended hard work, private property, low taxes, and social hierarchy. More intransigently antiparliamentary, George Valois's *Faisceau* foundered when wealthy backers withdrew their support. More opportunistic and less threatening, Pierre Taittinger's *Jeunesses Patriotes* adjusted better. But all produced more sound than fury; none showed revolutionary or charismatic proclivities; and none came anywhere near accession to power.

The penultimate chapter lists the reasons for the leagues' failure, not least of which was the lower level of social and economic dislocation in France than in Italy or Germany. Soucy sensibly attributes the chief responsibility for the waning of the leagues to Raymond Poincaré's reestablishment of the economy and his stabilization of the franc in late 1926. A second wave of fascism, arising with the depression of the 1930s, will be the subject of another study.

Moralistic, simplistic, woodenly written, and studded with literal translations, Soucy's synthesis is nevertheless illuminating. It sheds light on the jealousies and tensions rife among competing movements; their inability to invent a truly popular leader; their tergiversations on issues such as anti-Semitism and parliamentary participation; their floundering pugnaciousness; and their closeness to mainstream conservatism. Despite original research and useful recapitulation, little of this is new. But Soucy's major purpose is to establish once and for all that movements such as the *Action Française* and the *Jeunesses Patriotes* were neither leftist nor centrist but truly fascist. He does well on the leftism, less so on the Sternhell thesis "ni droite, ni gauche." In my reading he shows the subjects of his study to have been antidemocratic, authoritarian, sporadically antiparliamentary, and indifferent to the "violation of civil liberties" that accompanied Fascism in Italy. They also admired Mussolini. So, at the time, did Churchill.

I agree with Soucy that the chief difference between fascists, "fascists," and overt conservatives may have been more psychological than ideological. But, then, why not have recourse to René Rémond's political families of the Right and call these chaps "Bonapartist"?

EUGEN WEBER
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MARGARET L. ROSSITER. *Women in the Resistance*. (Praeger Special Studies.) New York: Praeger. 1986. Pp. xii, 251.

Margaret L. Rossiter seeks to fill a notable gap in the literature on the French Resistance by describing the work of women in escape lines, intelligence reseaux, clandestine newspapers, and paramilitary groups. Rossiter argues that, although women were more prominent in some types of resistance activity than in others, they made significant contributions in all spheres. In the process women exhibited "new-found talents as organizers and managers" as well as served with distinction in "so-called traditional activities" (p. 220). Women were particularly evident in escape lines and intelligence reseaux; 40 percent of the members of escape networks and 18 percent of the members of the intelligence organizations that Rossiter studied were women. In addition to recounting the actions of female resisters through a series of fascinating personal narratives, the author explores the relationship of women's participation in the Resistance to their receiving the vote in 1944. A separate chapter ("Six Who Loved France") details the actions of six American women in the French underground.

Despite quotations from René Cerf and Fernand Grenier that women's participation in the Resistance "was decisive" in gaining the vote for women in France, the author's contention that "their participation in the resistance was a major factor" is not demonstrated convincingly here (p. 224). As Rossiter notes, the Chamber of Deputies had consistently favored women's suffrage since the aftermath of the First World War. Only the indirectly elected Senate (especially the Radical senators) had blocked the vote for women in the interwar years. The marked demise of the Radical party and a sharp, leftward political swing at the liberation virtually guaranteed women's suffrage with or without the active participation of women in the Resistance. Hence, the link between activism in the Resistance and women's suffrage is perhaps not so direct as Rossiter implies. My argument is not so much that

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the author is wrong but that she has not proved her case. To make too much of this point, to which the author devotes, after all, only two pages of her text may be unfair, but to suggest in the preface and the conclusion, as well as on the book's cover, that the relationship of action in the Resistance and enfranchisement is a major theme of the book was perhaps unwise.

As regards the principal focus of the text, Rossiter has succeeded very well in her efforts to highlight the various roles played by women in the Resistance. The martyrdom of Bertie Albrecht and the exploits of Lucie Aubrac are certainly as worthy of recognition and as daring as those of Jean Moulin and Henri Frenay, whose stories are much more widely known. As one surveys the literature on the French Resistance, the author's assertion that women have failed to receive credit proportionate to their contribution seems justified, and this book takes a significant step toward filling that gap. Moreover, the book rests on an impressive amount of original research. The author has interviewed one hundred former resisters and corresponded with one hundred others, and, in the National Archives in Washington, D.C., she has discovered valuable and previously unused materials on the escape networks. Whereas most of what Rossiter writes about the general development of the Resistance and its various components covers familiar ground, her account of the operation of the escape lines is original and highly informative, and she provides excellent insights concerning problems associated with producing the underground press.

Because the book is entitled *Women in the Resistance*, one occasionally wishes for further development of intriguing themes—such as the contrasts noted in attitudes toward women in French, British, and American intelligence organizations—or more direct analysis of what was distinctive about women's roles in the underground struggle. But these are minor objections. Overall, the reader is captivated by the dramatic episodes told so effectively through the experiences of these valiant women, and one applauds the emergence of women in one more historical era from which a full public record of their accomplishments has been missing.

JOHN F. SWEETS
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EARL E. ROSENTHAL. *The Palace of Charles V in Granada*. Princeton: Princeton University Press. 1985. Pp. xvi, 320. $77.50.

The palace of Charles V in Granada has long been recognized as an extraordinary but enigmatic building. Much of the enigma is expressed in this question: how did a mature example of the High Renaissance style come to be built at a time when much architecture in Spain was just beginning to emerge from the Middle Ages? Earl E. Rosenthal's answer to this and other questions can be regarded as definitive and makes the reading of his book a rewarding experience.

This assessment does not mean that the book will be easily accessible to all. Rosenthal's analyses of the history of construction and the origins of the style are often microscopic and are mainly intended for an audience of historians of Renaissance architecture. It is safe to say that four of the seven chapters offer more detail than the nonspecialist may require, despite the author's clear and graceful style. Fortunately, he never loses sight of the heart of the matter as he dissects the evidence.

Much of the dense argumentation is devoted to establishing the original design of the architect Pedro Machuca and the extent of his responsibility for the execution of the never-to-be-completed palace. The story begins in June 1526, when Charles V and his bride, Isabella of Portugal, sojourned at the Alhambra. Enthralled by the beauty of the site, Charles determined to build a palace and delegated the responsibility to Luis Hurtado de Mendoza, a member of the noble family distinguished for its patronage of arts and letters. Hurtado's choice of architect is surprising because Machuca was a painter who had spent time in Rome earlier in the century. The palace of Charles V was his only architectural work.

In chapters 2 and 3 Rosenthal painstakingly reconstructs the work done under Machuca's supervision until his death in 1550 and then under his son and faithful follower, Luis, until 1571. The post-Machuca period, a long, unhappy one that stretched from 1572 all the way to 1923 and involved major changes in the design, is the subject of chapter 4. For those inclined to skim these sections, informative figures in the text graphically summarize the principal conclusions. Having established for the first time the extent of Machuca's contributions to the structure, Rosenthal devotes chapter 5 to studying the precedents and analogies of the design. Approximately seventy elements are analyzed to determine the sources of Machuca's style, which are located in Tuscan theory and Roman practice of circa 1500–20, specifically in the circle of Raphael. This bald summary of the conclusions, however, does not do justice to the exhaustive evaluation of Machuca's artistic origins.

The final chapter, entitled "The Significance of the Palace," is a grand finale to the book. In the first part Rosenthal offers a brief but thought-provoking reassessment of High Renaissance architecture and considers the palace of Charles V in the context of the diffusion of Italian style to other parts of Europe. He assigns a preeminent place to Spain in this

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process. The book concludes with a telling analysis of the palace as an expression of the "iconography of rulership," which convincingly links Renaissance ideals of princely virtue to the architecture and also provides detailed interpretations of the figural and emblematic ornamentation.

This book sets a high standard for the study of Spanish Renaissance architecture, a field that just now is enjoying a renaissance of its own. The full impact, however, can be measured only when read with the author's earlier study, the equally exemplary *Cathedral of Granada* (1961). In these works Rosenthal has reconstructed the history of two important Renaissance buildings designed to express the triumph of the Christian church and prince over the infidel. Rome may have been the center of the faith, but Granada was the frontier. In Rosenthal's books we can see how the cathedral and palace became monumental repositories of the religious, political, and cultural aspirations of imperial Spain.

JONATHAN BROWN
New York University

ALFRED HOWER and RICHARD A. PRETO-RODAS, editors. *Empire in Transition: The Portuguese World in the Time of Camões*. Gainesville: University of Florida, for the University of Florida Press and the Center for Latin American Studies, Gainesville. 1985. Pp. xiv, 234. $25.00.

In 1980, the fourth centenary of the death of Portugal's great epic poet, Luís Vaz de Camões, scholars momentarily refocused their attention on the Portugal of the discoveries and overseas expansion so out of vogue these days. Camões's Portugal attained the zenith of its imperial and commercial power in Africa and Asia about 1520. Portuguese letters also flourished, as Portuguese compared themselves and their deeds to the heroes and deeds of antiquity. In the 1550s, however, many Portuguese already were complaining of the universal corruption of officialdom, of rural depopulation, of declining food production and overseas trade—the symptoms of serious national malaise. Subsequent events appeared to confirm that malaise. In 1578 the military disaster at Alcázar Kebir decimated the aristocracy; the extinction of the Portuguese royal line in 1580 was followed by the loss of independence and by sixty years of Spanish domination. Culturally, the Counter Reform movement captured command of educational and political institutions. These forces of obscurantism in turn imposed censorship and used the Inquisition to silence or put to flight the few lights of Christian and pagan humanism in the land.

As their contribution to the Camões commemoration, Alfred Hower and Richard A. Preto-Rodas have selected for publication eighteen papers presented at the thirteenth annual conference of the University of Florida's Center for Latin American Studies. The editors present the papers in four sections. The first serves as an introduction to Portugal's intellectual climate and social and political institutions. A. H. de Oliveira Marques's paper presents an Italian cleric's view of Portugal in the sixteenth century and is especially appropriate for the editors' introductory purpose. Preto-Rodas's own contribution briefly considers the rationalist tradition in Portuguese humanism and the thought of Uriel da Costa, a Portuguese New Christian who influenced Spinoza. The second and third sections are devoted to the *outre mer*. George Winius's discussion of the decline of Portugal's Asian empire in the light of some recent scholarship and Joseph C. Miller's provocative assault on the myth of Lusotropicalism in Portuguese colonial achievements in Africa are notable offerings. José Honorio Rodrigues's study of the Portuguese language's difficult triumph over native American and imported African languages in seventeenth- and eighteenth-century Brazil stretches the volume's parameters well beyond Camões's lifetime. The final section, "Camões: A Man for All Centuries," is the centerpiece of the volume. In it the authors join the familiar debate over the relative preponderance of positive or pessimistic messages in Camões's *Lusiadas*. José da Silva Dias, Jack E. Tomlins, and Graça Silva Dias in particular explore the contradictions in Camões's work and in Portugal's era of apogee and rapid decline. Graça Silva Dias offers a brief sketch of the social and political circumstances that shaped Camões's work.

I have no quarrel with the editors' premise that sixteenth-century Portugal was a nation in transition; the papers amply reflect the symptoms of crisis and change in Portuguese society. Nevertheless, the papers fail to define the larger historical processes at work. For instance, they only discuss superficially the commercial expansion and its impact on Portuguese society and economy. The papers also do not tell us how Portugal was changed in the transition. The authors' and editors' almost exclusive reliance on literary evidence restricts their view to a small sector of Portuguese society—the handful of literati and officials drawn from the aristocracy. The Portuguese economy experienced a significant commercial expansion in the early sixteenth century, and these representatives of the landed and soldierly classes were the very elements for whom the expansion caused the greatest trauma. Totally absent from consideration here are the elements that benefited by commercial expansion—the small but prosperous urban commercial classes. A more comprehensive view of Portuguese society, taking into account all these elements, might challenge the

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scenario of general decline in the second half of the sixteenth century. One need only recall that Brazil's first sugar boom—a major boost to Portugal's maritime economy—began about 1580.

Although the editors have drawn together a varied and lively collection of papers, this volume will disappoint the reader expecting some indication that, after several decades of neglect, scholars have begun revising the history of Portugal's century of greatest promise and greatest disappointment.

JAMES C. BOYAJIAN
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FRANCISCO VILLACORTA BAÑOS. *El Ateneo científico, literario y artístico de Madrid (1885–1912)*. Foreword by MANUEL ESPADAS BURGOS. Madrid: Consejo Superior de Investigaciones Científicas or Centro de Estudios Históricos, Madrid. 1985. Pp. xvi, 382.

From its establishment in 1835 to its seizure by the Franco regime one century later and incorporation into the official Propaganda Section, the Athenaeum of Madrid attempted to fulfill that unique social role of its Greek and Roman prototype: to promote dialogue between the world of learning and the world of public affairs. A private institution (albeit licensed by the government in accordance with Spanish legal norms) with an elected board of directors, the Athenaeum performed three functions: it was a social club for a self-selecting group of middle-class professionals, a postgraduate academy where classes and lectures kept members up to date on scientific research and artistic developments, and a forum for open debate about public policy issues.

Francisco Villacorata Baños is most interested in how the Athenaeum functioned as a forum during the "restored" Bourbon monarchy, from the move into handsome quarters on the Calle del Prado (just around the corner from the Cortes) to the visible disintegration of parliamentary government following the assassination of José Canalejas. Villacorta's earlier book dealt with Spanish intellectuals from 1808 to 1931. Here he considers the specific function of an intelligentsia and examines the Athenaeum membership as a social group consciously assuming the role of a vanguard in the formation of public opinion by disseminating knowledge acquired through professional organizations and classrooms. Influenced by Antonio Gramsci's concept of hegemony and by contemporary sociologists working on the problem of how intellectuals persuade the governed to accept the values of those who dominate the state, Villacorta identifies the Athenaeum's targeted "public" as the middle strata of the bourgeoisie. Whether under monarchy or republic, Spain's liberal parliamentary system relied on this social group, which was, however, dangerously isolated from the financially powerful oligarchy and from the demographic force of the workers. Fortunately, the theoretical framework is detached from the factual account of intellectual pluralism and effective discourse by men and women, Catholics and secularists, monarchists and republicans, and enlightened army officers. They debated divisive issues such as regionalism, military reform and colonialism, an underdeveloped economy, labor movements, and, as elsewhere in Europe at the time, the fundamental problem of how to reconcile individual rights with society's requirement of order and stability. The last was not a metaphysical but a pragmatic concern, coinciding with preparation of the final text of the civil code that formally "closed" the debate. It coincided also with the disastrous military defeat in 1898, which called into question all society's norms.

But interest in these debates died as class conflict intensified, decreasing the influence of the Athenaeum's narrow social base. Its function as an intelligentsia declined as scholarship, more specialized and more expensive, retreated to government-financed research seminars and laboratories. Villacorta argues that, after 1912, the audience of the Athenaeum no longer contributed to the formation of policy by debating issues but passively listened to lecturers seeking publicity and a political constituency, who presented specific policy recommendations in a semischolarly format.

The book contains much repetition and some contradictions because several chapters were published as articles and included without modification, a minor caveat. Villacorta reconstructs in an appendix a detailed chronology of the activities of the Athenaeum for the period (invaluable given the disappearance of its archive) and provides a complete index of names and an extensive bibliography.

JOAN CONNELLY ULLMAN
University of Washington

RICHARD GUNTHER *et al. Spain after Franco: The Making of a Competitive Party System*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xix, 516. $42.00.

The democratization of Spain since 1975 has attracted a great deal of attention from journalists, historians, political scientists, political sociologists, and writers on international affairs. Several comparatively brief historical accounts have already appeared in English, and the bibliography in Spanish of political journalism, memoirs and historical accounts, and collections of electoral and other data is growing very large. The present work by Richard Gunther *et al.* is not, however, another history of the Spanish transition but a highly original and detailed

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analysis of the development of the new political party system that emerged in the elections of 1977 and 1979.

After a brief introduction, the book treats the emergence of the new parties in 1976–77 and their further evolution in 1978–79. One chapter is devoted to social class and the general election of 1979 and another to religion and politics. Two of the most complex and illuminating sections deal with the structure of competition and the role of leadership for the Left and the Right and with micronationalism and the regional party systems of Euskadi, Catalunya, and Galicia.

The core of the research is drawn from a rich and multifaceted survey of approximately fifty-five hundred voters from all major districts and regions at the time of the 1979 elections, supplemented by extensive interviews with national and provincial party leaders between 1978 and 1983. With the addition of standard published primary sources and secondary works, the result is an extraordinarily rich data base that in some respects may be compared with major Spanish collections such as the *Informe sociológico sobre el cambio político en España 1975–1981* (1981) or *Atlas electoral del País Vasco y Navarra* (1981) by Juan Linz *et al.*

These materials are systematically probed to provide a careful analysis of the main aspects of party formation, party differentiation, electoral performance, and relationship to social classes and ideological divisions between the Left and the Right. The changing relationship of religious values to partisan politics is made much clearer, and probably the richest aspect of the entire study is the lengthy treatment of the cleavage between the center and the periphery and the relationship of social sectors, ideological orientations, and a number of other key political variables to the new party structure and electoral performances in the three primary exceptional regions.

The result constitutes one of the deepest and most extensive analyses in any language of the new Spanish party system and its regional, social, economic, and cultural bases. It provides not merely "the best book in English on Spanish politics after Franco," as Juan Linz has said on the dust jacket, but also the most useful and concentrated single study in any language on the character and dynamics of political partisanship during the key transition years of 1977 to 1979. The book will be indispensable not merely to students of contemporary Spanish history but also to those concerned more generally with Spanish politics, the affairs of Southern Europe, and new democratic transitions.

STANLEY G. PAYNE
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KAARLO WIRILANDER. *Herrskapsfolk: Ståandspersoner i Finland 1721-1870* [Gentlefolk: The Upper Class Persons in Finland, 1721–1870]. Translated from Finnish by EVA STENIUS. (Nordiska Museets Handlingar, number 98.) Stockholm: Nordiska Museet. 1982. Pp. 465.

In a book hailed by Finnish historians as the "event of the year" in 1974—here published in Swedish in 1982—Kaarlo Wirilander undertakes to examine collective social attitudes and measure statistically how these, and the changes they underwent, were reflected in demographic structures through the century and a half between the corporate society at its height and its dissolution in Finland. The closest analogy is Sten Carlsson's work on Sweden.

Finland is well suited for such analysis because of its small but growing population and limited numbers of elites. The elites comprised 10,885 people in 1728 and 26,905 by 1870, declining from 3.4 percent to 1.5 percent of the total population. The country was administratively uniform under the Swedish monarchy before 1809, thereafter becoming a unified, autonomous grand duchy under the Russian tsar. Its public records were meticulously kept. The Finnish example is, moreover, broadly analogous to other Scandinavian and Continental developments.

Wirilander's treatment of ideas is by necessity based largely on common European traditions. He emphasizes the problems of rediscovering the self-evident—thus seldom directly expressed—assumptions of an era now covered with "layers of historic dust." His concluding chapter traces the evolution of the old corporate elites into a new, composite upper class defined through culture, deportment, and prosperity. Throughout, Wirilander re-creates the values of a vanished way of life in fascinating detail.

Chapters 3 and 4—the greater part of the book—give a quantitative analysis of the elites, stressing the majority that were in state service, that is, in the military, clerical, judicial, civil, and municipal establishments. He studies the size, composition, opportunities, age and geographic distributions, marriage patterns, education, and recruitment of each, providing numerous graphs, tables, and appendixes.

Wirilander nonetheless leaves some significant questions unanswered. With his concentration on the quantifiable, he tells little about the most open, expansive, and mobile elements within the developing private sector. He includes virtually nothing on the impact of the often-bitter sociopolitical struggles in Swedish Finland between 1760 and 1809 or of the socially loaded conflict between "Fennomanes" and "Suecomanes" in the nineteenth century. Careers in Sweden and especially in imperial Russia are scarcely mentioned. Why Wirilander stops at 1870,

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rather than at the final demise of the estates system in 1906, is unclear.

Such observations aside, Wirilander has produced an admirable study, combining in rare fashion the skills of both humanist and social scientist with the insights of the historian.

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PETER BLICKLE. *Gemeindereformation: Die Menschen des 16. Jahrhunderts auf dem Weg zum Heil.* Munich: R. Oldenbourg. 1985. Pp. 234. DM 88.

HEIDE WUNDER. *Die bäuerliche Gemeinde in Deutschland.* (Kleine Vandenhoeck, number 1483.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 187. DM 18.80.

The term *Gemeinde* (community) is central to the works of both Peter Blickle and Heide Wunder, but the orientation of these two valuable monographs is quite different. Blickle deals with the Reformation, Wunder with rural Germany.

Blickle's book offers a fresh perspective on the early history of the Reformation. Luther's and Zwingli's ideas had such a strong appeal to common people in southwest Germany and Switzerland up to 1525 because there the Reformation was accepted as a reformation of the community, a *Gemeindereformation*. The early Reformation has been characterized as an urban event, but now we also learn of a powerful reformation movement in the countryside. Blickle discusses the various aspects of the rural reformation, centering on demands for preaching the gospel and for the appointment of pastors by the community. The first three of the Twelve Articles now appear in a new light, although Blickle bases his discussion on much broader documentary evidence. The appeal to the gospel and to divine justice also justified social and economic demands, especially the abolition of serfdom. Similarly, the early urban Protestants demanded the preaching of the pure gospel and the elimination of clerical privileges. The demand for autonomy in church affairs, not Luther's central doctrine of salvation by faith, evoked the strong popular response.

In 1523 Luther envisaged a new church structure, granting the community control over the appointment of ministers and over church affairs. But the *Gemeindereformation* in southwest Germany owed more to Zwingli than to Luther. Blickle considers Zwingli's affirmation of the community's right to resistance as the background to the Twelve Articles and the Peasant Revolt. Indeed, Luther's treatises against the rebellious peasants in 1525 were directed not only against Müntzer but also against Zwingli.

But why were the villages and towns in southwest Germany and Switzerland so receptive to the ideas of communal autonomy in church affairs? This region had been shaken by more than sixty uprisings in the fifteenth and early sixteenth centuries, and the emancipation from feudal society had lead to a relatively high degree of self-administration and civic responsibility. The modes of social and political behavior in town and village explain the receptivity to a reformation that gave control of the church to the community. The communal reformation was a reflection of the high degree of civic development in southwest Germany and Switzerland.

Blickle presents a grand thesis. His perception of the lines of development and his firm mastery of the factual information are truly impressive. The book is also quite readable; it is punctuated and enlivened by occasional outbursts of anger over the positions of other scholars. The description of the peculiar aspects of the rural reformation impressed me most.

Such a wide-ranging book also raises critical questions. The thesis is predicated on the unique social development of the region between Alsace and Salzburg. Will this assumption stand up to critical examination? I also wonder whether a social analysis of the early Reformation, even within the confines of Blickle's topic, can leave out the gigantic financial fraud perpetrated by the church on the eve of the Reformation. The author's few brief remarks on the Anabaptists are not always accurate. Maybe he should have looked more closely at those Anabaptist congregations in which a democratic reformation was carried through most consistently. Lastly, I wonder whether Blickle does full justice to Luther's arguments against rebellion. Luther's harsh condemnation of the peasants is easily criticized, but his alternatives in the circumstances are never examined.

Still, this is a book of fine scholarship and intellectual rigor. Its combination of theological and social analysis may change our approach to the history of the early Reformation.

During the past twenty-five years German scholars have produced an extraordinary number of works on German rural history. On the basis of these articles and monographs, Wunder presents a new interpretation of the history of the peasant community from the sixth century to the late nineteenth century. During this time span enormous changes occurred in rural life style and modes of work. She sees the close neighborly relations and the small size of the community as permanent characteristics of the rural community.

A long chapter dealing with peasant communities from the eleventh to the sixteenth century concentrates on the size of farms, dues and services, rights of inheritance, relations between farmers, and especially the relation of the village community to the landlords and the governments both in eastern Germany and western Germany. Emphasis is placed

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on the changes that occurred in the fifteenth century as a result of population losses and agrarian depression. Because the sources of the medieval period often are legal documents the discussion is at times difficult and abstract.

Much more colorful and rich is the picture of the peasant community from the sixteenth to the eighteenth century, when the new administrative machinery and the absolutist state drastically changed the relationship between the government and the village. The community lost many of its legal and administrative functions and was subjected to closer governmental control. The emergence of striking differences in the legal status of the peasants in eastern and western Germany is carefully discussed, in particular the rise of the *Gutsherrschaft* and its consequences for the economic, social, and legal position of the farmers in eastern Germany. Wunder attributes the weakness of the village population in relation to the landlord more to the devastation of the Thirty Years' War than to enclosure and the buying out of peasant holdings. We read of the changing positions of village courts, office holders, communal assemblies, of the structure of the rural population, and of conflicts within the village community over the status of the growing number of landless peasants.

A final chapter discusses the effects of the *Bauernbefreiung* of the early nineteenth century and underlines the harmful economic consequences of the emancipation for the east German farmer. Equally illuminating are the remarks on the changes that nineteenth-century bourgeois society brought about in the village community. I was particularly intrigued by the section dealing with the interrelations of the various social groups in the village community. Whereas in the medieval rural community personal relationships defined rights and obligations between farmers, the nineteenth-century village increasingly became an administrative unit—a place of residence where only part of the population was still engaged in farming. Obviously, many more problems are discussed, including the abolition of the rights of landholding farmers to common land and religious divisions in the village.

The book presents an exceptionally instructive, well-structured account of the political and social development of the rural community over one thousand years. Wunder adds an appendix on the approaches taken by nineteenth- and twentieth-century scholars in studying village community. Of course, the presentation is concise. At times the brevity imposed by the publisher diminishes clarity. I am sure the author could have said much more, and with every page the value of the book would have increased. Still, anyone interested in German social history, particularly rural history, will greatly profit from reading this informative book. The richness and diversity of topics covered will not fail to stimulate new lines of research.

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KURT WESOLY. *Lehrlinge und Handwerksgesellen am Mittelrhein: Ihre soziale Lage und ihre Organisation vom 14. bis ins 17. Jahrhundert.* (Studien zur frankfurter Geschichte, number 18.) Frankfurt a/M.: Waldemar Kramer. 1985. Pp. 416.

Kurt Wesoly has written a fine monograph on apprentices and journeymen in the cities of Speyer, Mainz, Worms, and Frankfurt between 1300 and 1700. Despite its title, this work concentrates on the material and moral conditions of life among that group of mobile, dependent laborers, the journeymen.

The principal conclusions of this study will surprise some scholars. First, Wesoly demonstrates convincingly the geographic mobility of journeymen. Wandering from city to city in search of work was both the essence of life for these men and the impetus for the creation of their fraternal organizations. Insofar as the *Gesellenschaften* discharged important social and economic functions, especially policing members and finding them work, guilds and governments tolerated their existence. Indeed, despite the well-known conflicts and prohibitions that characterized the relations between journeymen organizations and the larger communities of which they were a part and despite the latent threat posed to the commonweal by the capacity of journeyman to orchestrate work stoppage, masters and magistrates made no concerted effort to disband them. Wesoly concludes that relations between independent and dependent workers and between governments and *Gesellenschaften* rarely assumed desperate, existential proportions. Second, the journeymen of the Middle Rhine demonstrated a high degree of solidarity, which seems all the more surprising given their high degree of vertical mobility. Even though journeymen in the cities examined by Wesoly could become masters with relative ease, they viewed themselves as distinct and distinguished groups. Solidarity was, according to the author, a consequence of that same peripatetic existence and those same fraternal organizations. The council, aid, and camaraderie shared by journeymen of the same trade was extended and reinforced by the shared rituals of the *Gesellenschaften*. Likewise, the *Gesellenschaften*, through their authority over members, could enforce common standards of behavior and common economic actions.

This study of journeymen falls between two great historiographic traditions. On the one hand, mate-

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rial life among dependent laborers as an object of study parallels the concern of a growing body of English and French scholarship with journeymen and *compagnonnages*. On the other hand, Wesoly's exhaustive archival research, which concentrates almost exclusively on legal and governmental documentation, indicates the close kinship of his work to the German tradition of guild scholarship.

Such problems as confound this work grow out of its association with these two traditions. *Lehrlinge und Handwerksgesellen* would have been much strengthened through greater attention to the methodologies and debates current among French and English scholars; readers will be surprised and dismayed by the almost total absence of this scholarship from Wesoly's bibliography. Although generally cautious in the interpretation of traditional archival sources, the author seems to chafe occasionally under their limitations. For example, his argument that the essential lack of hostile confrontations resulted from a shared organic vision of society and labor, in which master and journeymen were necessary, mutually dependent components, is less than convincing given its dependence on legal and institutional documentation. Ultimately, questions of *mentalité* require different sorts of sources.

Such problems, however, appear infrequently and do not detract from the work as a whole. Wesoly has contributed a valuable regional history of dependent labor before industrialization.

THOMAS MAX SAFLEY
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JOACHIM WHALEY. *Religious Toleration and Social Change in Hamburg, 1529–1819*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1985. Pp. xi, 248. $49.50.

That religious intolerance remained a strong influence in German local politics even after 1648 is a theme not often elucidated by historians who study the era between the Wesphalian settlement and the end of the old empire in 1806. Joachim Whaley traces the story of Hamburg's religious disputes from the introduction of the Bugenhagen Ordinance in 1529 to 1819, when the Act of Vienna was implemented.

Whaley emphasizes the use of original sources and has thoroughly sifted papers of the Hamburg senate and of several of the key collegia, such as that of the aldermen and the Lutheran ministry. I am particularly impressed by the range of documentation, which includes diplomatic correspondence in the Public Records Office as well as records of the Edzardi proselytizing center. The result is an important, excellently researched study of a difficult theme: intolerance versus gradual toleration. Many historians of this era have emphasized the growth of Enlightenment attitudes toward religion and have played down the continued intolerance of the mass of the people after 1648. Reason of state certainly dominated the considerations of princes but not those of the bourgeoisie or the commoners. In Hamburg concessions were made to the Calvinist and Jewish minorities by the eighteenth century, largely in order to keep these communities from living in nearby Danish Altona, which guaranteed religious freedoms to all.

The Hamburg minorities repeatedly attempted to secure the right to worship in public, to build their own church or synagogue, and to celebrate baptisms, weddings, and funerals in Hamburg rather than in Altona. Both Prussian and Dutch envoys protected the Calvinists and allowed them to worship in the private chapels of their residences.

The hatred of outside religions remained intense: as late as 1719 an anti-Catholic riot occurred. Fear of the Jesuits who served the schools and the private chapel of the imperial envoy remained strong. Although the Sephardic Jews who had come in the 1550s had a synagogue at one time, the much larger German-Jewish population of the eighteenth century did not.

By the end of the eighteenth century, C. W. Dohm and other Enlightenment writers had developed, as Klaus Epstein's researches pointed out, support for Jewish emancipation. Whaley's work adds much here, for he has studied the writings of the Lutheran Seniors and shows that the fanatical Johann Melchior Goeze, who was less harsh than his predecessors, in a somewhat disgruntled spirit attributed the senate's interest in protecting the Jewish community to its "mercantile cupidity."

The role played by the transformation of mentality in changing an unyielding and biased social attitude is well documented here. All present research on problems of religious conflict will be stimulated by Whaley's study of how group tension and responses to threats to an established religious elite might have been reduced by the diffusion of new ideas about those religions.

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ROBERT R. TAYLOR. *Hohenzollern Berlin: Construction and Reconstruction*. Port Credit, Ontario: P. D. Meany. 1985. Pp. xiii, 227. $35.00.

This slight book portrays Hohenzollern architectural ambitions in Berlin and the fate of their monuments after 1945 in the former and now unhappily divided capital. As Robert R. Taylor shows, the Prussian kings usually took an active, but not always commendable, part in planning the city

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and its buildings. Some Hohenzollerns had defective taste, and none, except for Frederick the Great (to whom edifices were "my dolls"), was really knowledgeable about architecture. Many Prussian rulers were stringently parsimonious, insisting on the employment of existing foundations, and this niggardliness resulted in buildings that were awkwardly sited (St. Hedwig's Church) or cramped in design (the Royal Theater). But the Hohenzollerns often had a knowing eye for architectural talent, however, and the city they commissioned could boast many public buildings of exemplary design.

Berlin has been fortunate in recent years in that authorities in both West and East have restored many of the Hohenzollern monuments with considerable external fidelity. The only major casualty, one that may be regretted historically but not architecturally, was the demolition by the German Democratic Republic in 1950 of the substantial remains of the Royal Palace, a stolid eighteenth-century building, noble in detail but later defaced by a hideous octagonal dome. This act of destruction, Taylor persuasively argues, proved a sufficient propitiation to ideological purity, enabling the Marxist authorities, now imbued with proletarian virtue, to proceed to rebuild other, even ostensibly military, monuments of Berlin's royal past.

Taylor's concern is with the dynasty's great ceremonial structures and not with more utilitarian, but often grandiose, buildings such as railroad stations, which were also emblems of monarchical pretension. Hohenzollern dreams and plans that did not materialize (the Gillys receive inadequate attention) and the Prussian rulers' anxiety to surpass the Wittelsbachs and Habsburgs, who were busily at work ornamenting Munich and Vienna, warrant scrutiny. The last Kaiser, Wilhelm II, who was convinced that his universal talents embraced architecture, ought to figure more prominently, especially in connection with the Reichstag building. Wilhelm I commissioned this gigantic palace, but it fell to his grandson to erect what he was pleased to refer to as the *Reichsaffenhaus*. For both ideological and artistic reasons, Wilhelm II despised the building and subjected its architect, Paul Wallot, to much abuse, not all of it deserved. The Kaiser also did not much like Julius Raschdorff's pompous cathedral, which he unwillingly had built according to a scheme promoted by his father, Kaiser Friedrich III. Wilhelm II much preferred the work of Alfred Messel and Ludwig Hoffmann, neither of whom is considered here. An unanswered question is the degree of influence (if any) that the last German sovereign exercised on private construction in his capital, in which the largest building, one that embarrassingly dwarfed the neighboring Hohenzollern temples, was an ungainly counting house erected by the Dresdner Bank.

The Hohenzollerns and their architects suffered from a singularly unfortunate incapacity to deal successfully with domes. St. Hedwig's is grotesquely bulbous, those crowning the French and German churches too spindly, and the protuberance rising from the roof of the Charlottenburg palace resembles a muffineer executed in stone. But even these buildings are not without compensatory virtues, and the great structures of Friedrich Schinckel are clearly products of profound architectural genius. The great structures of Andreas Schlüter command considerable respect. Even in the wake of war, little imagination is required to realize the Hohenzollerns' accomplishment in raising a city of handsome buildings. The architecture of royal and imperial Berlin, palely sketched in this brief book, deserves a rendering that possesses a measure of the grace and monumentality of the subject. More would not be less.

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KARL ERICH BORN. *Wirtschafts- und Sozialgeschichte des Deutschen Kaiserreichs 1867/71–1914*. (Wissenschaftliche Paperbacks, Sozial- und Wirtschaftsgeschichte, number 21.) Wiesbaden: Franz Steiner. 1985. Pp. 175. DM 24.

This book is intended in the first place as a textbook, not as a new interpretation of the economic and social history of imperial Germany from 1867–71 to the First World War. It is comprehensive, full of detailed information, and most useful as a guide through a vast, often controversial literature on the subject. After a comparatively brief assessment of the global demographical data and the migration movements during this era, Karl Erich Born gives brief but concise accounts of the performance of the various sectors of the economy, such as agriculture, mining, industry, and banking. The period is characterized by the transformation from an agrarian to an industrial society. In this context Born emphasizes that Germany history up to the present has had more continuity on the social and economic levels than in the political sphere.

The presentation is solid and informative, though in a way conventional, leaning toward a conservative interpretation. Born, however, works hard to assess objectively the changes in the social structure in this period and to give due attention to the fortunes of the working class. The third part of the book offers an elaboration of governmental economic and social policies. Quite impressive is his rather positive assessment of social policies under Bismarck and Wilhelm II. Wilhelmine imperialism, both formal and informal, is dealt with briefly but informatively, although its significance is played down, especially

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when the author puts it in an international context. Accordingly, Born arrives at a mildly critical, though sympathetic, evaluation of imperial Germany. He believes that, although the country suffered from remnants of premodern social structures and forms of social conduct, it was on the whole just as liberal and no less respectable than many other states in Europe at that time, a point on which other scholars like myself are likely to disagree.

WOLFGANG J. MOMMSEN
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VOLKER EICHLER. *Sozialistische Arbeiterbewegung in Frankfurt am Main 1878–1895*. (Studien zur Frankfurter Geschichte number 17). Frankfurt a/M.: Waldemar Kramer. 1983. Pp. 456.

In one special way, Frankfurt am Main is an excellent case study of German social democracy before World War I. The city was a stronghold of the bourgeois democratic movement during the unrest of 1848–49, and it continued to have an influential progressive bourgeois faction in the decades that followed. Thus, in Frankfurt the Social Democratic party (SPD) regularly confronted its toughest competitors for the allegiance of workers: the progressive left liberals. Almost 75 percent of all run-off elections won by the SPD in the thirteen elections of the Second Reich pitted socialist candidates against various shades of left liberals. This situation tested the intransigent ideology of the SPD and challenged the Frankfurt party organization to hold the line in theory while wooing workers away from the liberals.

Volker Eichler has dealt successfully with this and many other aspects of the socialist workers' movement in Frankfurt. His well-written study is a masterpiece of research (there are over twenty-three hundred footnotes for just under four hundred pages of text), on the same level as the extremely high standards of scholarship set by other German works in the field. Eichler stands apart from many of his German contemporaries who have written on the social democratic movement because of the felicity of his style; this book will be a pleasure to read for anyone who has suffered through the rather labored prose of Dieter Groh and his ilk.

Eichler's book is also a valuable contribution to the continuing destruction of the myth of a monolithic party in Germany prior to 1914. Certainly the Germans themselves, at least the thoughtful among them, have long been aware of the doctrinal and personal disputes that occurred in nearly all branches of the SPD, but Eichler's carefully reasoned, well-presented study will help convince doubting outsiders as well.

The story of the Frankfurt movement during these years is certainly not one of steady, heroic growth of a united movement in the face of a uniformly hostile world. The nearly seventeen years covered here include at least six distinct phases: a slow recovery after the imposition of the antisocialist law (October 1878); brief radicalization during the early 1880s; destruction of the radical element when the authorities instituted repressive measures following the assassination of a leading police figure in January 1885; a split that developed when Karl Frohme, a moderate Reichstag representative, alienated much of the membership; a brief flirtation with the so-called *Jungen* radicals and with anarchism in the early 1890s; and, finally, the clear victory of the moderates, signaled in the summer of 1896 when Max Quarck, a leading reformist intellectual, became editor of the Frankfurt party paper, the *Volksstimme*.

This book is mostly for experts because of its detail and narrow focus. It is an admirable work nonetheless and makes a major contribution to the local studies that are now giving a firm foundation to our understanding of the prewar SPD.

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CAROLE FINK *et al.*, editors. *German Nationalism and the European Response, 1890–1945*. Norman: University of Oklahoma Press. 1985. Pp. xv, 299. $19.95.

This collection of essays is in effect a *Festschrift* for Hans Gatzke, "an expression of his students' esteem and affection" (p. 267), and a very distinguished tribute it is.

In her original and imaginative contribution, Isabel Hull writes of William II's efforts to adapt Prussian dynastic ritual to embrace German nationalism and appeal to a broader spectrum of society. William did so with his usual heavy hand by introducing luxury and ostentation that conflicted with traditional Prussian values of austerity and frugality and by creating a veritable inflation in honors and awards that vulgarized and cheapened the very values he sought to enhance.

William II's almost inconceivable lack of political judgment is well illustrated in Lamar Cecil's essay on the monarch's personal diplomacy, in this case his heavy-handed efforts to curry favor with his imperial Russian relatives, or "colleagues" as he chose to call them. His protective and patronizing attitude toward Nicholas II aroused feelings of suspicion and distaste, and his behavior in general aroused contempt. "He thinks he is a superman," said Alexandra, "but he's really nothing but a clown" (p. 118).

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Erich Hahn analyzes how, after 1918, the bureaucrats of the German Foreign Office handled the war guilt issue and built up a "preemptive historiography" denying German responsibility for the war. He believes that through the 1920s the anti–war guilt propaganda of the Foreign Office stifled a critical reassessment of Germany's role in Europe and possibly postponed a break with the traditions and aspirations of imperial foreign policy. Whether official historiography was as influential as Hahn suggests is open to question, but he is certainly correct in emphasizing its importance in maintaining continuity in German political culture.

In dealing with a very different officialdom—Germany's Jewish leadership—Herbert Levine reminds us that the German Jews were the first victims of radical German nationalism and of their own tragic failure to comprehend the nature of that threat. They attempted to create an organization with which the Nazis could and would negotiate, but they only created an instrument for their own destruction.

In her essay on the Genoa Conference of 1922 (and its unintentional by-product, the Soviet-German Rapallo agreement), Carole Fink maintains that the key issues were to bring the Soviet Union back into the system, establish a viable reparations program, and construct an international system that would stabilize governments, revive prosperity, and allow for lower tax burdens. Following the failure of David Lloyd George's grand design to achieve these purposes, Britain opted for partial solutions that undermined the Paris peace treaties without substituting a durable structure of European security.

Edward Keeton deals with another failure, Aristide Briand's German policy from 1925 to 1931 and his efforts to construct a diplomatic system that would compensate for France's apparent vulnerability to Germany's greater military and economic potential.

Williamson Murray discusses Germany's Blitzkrieg strategy, which he believes developed out of tactics employed in the final months of the First World War. Thanks in large part to the German army's flexibility and willingness to innovate, this strategy was developed further in the interwar years. Although advocates of an army of mobility had trouble convincing doubters, they finally carried the day and created forces that had the ability to concentrate rapidly, launch sudden blows, and exploit breaches in enemy lines. Not Hitler but the German generals developed the strategy that came perilously close to winning the Second World War.

MacGregor Knox deals with the rather less successful military leadership of Italy. He accuses all branches of the Italian armed forces of intellectual rigidity, doctrinal backwardness, and sheer incompetence, the result of grave deficiences in Italian methods of recruitment and training. Believing that Mussolini's militant bombast was all a colossal bluff, Italian military leaders were unprepared to deal with problems of combat when their leader actually plunged them into war.

Fink and Annelise Thimme have provided a survey of Gatzke's career and scholarly work.

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S. J. LEWIS. *Forgotten Legions: German Army Infantry Policy, 1918–1941*. Foreword by CHARLES BURDICK. (Praeger Special Studies.) New York: Praeger. 1985. Pp. xviii, 189. $29.95.

Military history continues to move away from its traditional operational focus and interpret war less in terms of armies than in terms of the social, economic, and moral structures sustaining those armies. This generates corresponding indifference to the nuts and bolts of war. Questions of tactics and doctrine, when they are considered at all, are usually left to soldiers with scholarly pretensions. It is as though students of slavery were to dismiss the everyday operations of the Old South's plantation system, or historians of politics to ignore the conduct of elections. S. J. Lewis promises a corrective in an important field. The Wehrmacht's often-cited fighting power depended less on the glamor branches, the air force and the tankers, than on the infantry. It was the foot soldier whose endurance and battle craft sustained the Third Reich long after Blitzkrieg was a memory and total war revealed the limits of Germany's military modernization. Moreover, such familiar authorities as Edward Shils and Morris Janowitz, as well as Martin van Creveld in his more recent analysis, credit much of the *Landser*'s formidable combat performance to moral factors. By proposing to concentrate instead on doctrine and training, Lewis takes his revisionism a welcome step further.

Unfortunately, the work's concept and execution do not sustain its premise. Its time frame is inappropriately narrow. The year 1918 is a political watershed, not a doctrinal one. Most of the Reichswehr's ideas on the nature of modern infantry had their roots in World War I and remain correspondingly incomplete in Lewis's presentation. Many of the German infantry's greatest achievements came after 1941. Were they products of improvisation or reflections of preparation? Lewis is silent on the question.

Lewis's arbitrary cutoff dates reflect the book's more fundamental weakness: it fails to maintain focus. Lewis is reluctant to grapple with details of tactics, organization, and armament. He says far too little about the evolving functions of the rifle and the

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light machine gun in the infantry squad; about problems of fire support and the accompanying debate over the control first of infantry cannon, later of assault guns; and about the infantry's expected and actual roles in mobile warfare. Instead, he wavers time and again toward such general, and presumably more interesting, subjects as the German revolution of 1918, the relationship of the Wehrmacht to National Socialism in the 1930s, and the strategies of the Battle of France and Operation Barbarossa. The infantry becomes a source of anecdotes rather than a subject of analysis. Because its center does not hold, the book collapses. Germany's foot soldiers remain neglected even by their intended chronicler.

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FRANKLIN C. WEST. *A Crisis of the Weimar Republic: A Study of the German Referendum of 20 June 1926.* (Memoirs of the American Philosophical Society, number 164.) Philadelphia: The Society. 1985. Pp. xiii, 360. $30.00.

By and large, Franklin C. West has written a political history of the referendum of June 20, 1926, in the Weimar Republic, which asked the German people whether the former reigning princes should be dispossessed without compensation. The story is a sad one, once more reflecting the partisanship, polarization, and confusion that characterized much of the political life of the ill-fated republic.

West has reconstructed the various stages of this history in a convincing manner. He introduces the problem by explaining the situation of the German princely houses after a revolution that had not been radical enough to liquidate the Second Reich's sovereign rulers and confiscate all of their property. In a new republican state system that perpetuated many of the pluralistic patterns of the federated Wilhelmian regime, no consensus arose about how to treat the by now jobless royalty. With the new central government in Berlin sitting idly by, responsibility for action for or against the princes devolved on the individual states. But only from some of those was legislation forthcoming, and much of it did not prove binding because it was challenged by the intended victims. Radically leftist states, such as Thuringia, were quick to expropriate, albeit only tentatively; a few even concluded compromises that appeared not to violate the monarchs' holdings. One weighty question was what to do with the Prussian possessions of the former Kaiser. Although streamlined legislation on the entire set of problems remained wanting, some princes took their own counterclaims to court, knowing that the overwhelmingly monarchist judges would favor them.

It is clearly not very interesting to learn about the preliminaries and the final result of the protracted political negotiations as such; that chain of events is in every textbook. After parliamentary and caucus wranglings and at the instigation of the German Social Democratic party (SPD), a national referendum was finally held in 1926, which demonstrated that not enough citizens favored outright expropriation. Thereupon, the individual states either fell back on previous compromises concluded with the princely houses before the referendum or, like Prussia, signed modified agreements with their princes, generally according them a fair share of their former wealth.

Thus to have recounted faithfully these already well known developments, in which the profile of the princes themselves remains unnecessarily bland, does not constitute the value of West's study. Rather, it lies in the way in which he has used the combined compensation and referendum issue as a kaleidoscope to analyze the Weimar parties at different times and from varying perspectives in the contest.

Hence, the SPD emerges as a committed party for the people that wished to do popular service by benefiting the former princely subjects after long-endured economic inequities without upsetting the princes unduly: the SPD believed that some recompense to the illustrious families surely was in order. As on other issues during the Weimar period, however, the SPD allowed itself to be overruled by the radical Communist claim for expropriation without compensation so as to secure agreement on launching the referendum in the first place. If the German Communist party is portrayed as the totally committed party on the Left, the German National People's party, irrevocably beholden to monarchy, appears as equally committed on the opposite side of the political spectrum.

The author is at his best when showing how vacillating and potentially divided the bourgeois parties in the Center really were. In so doing, he has indicated the path of those parties in the last years of the republic and later under Hitler. The Center party, "reactionary, interdenominational, and republican" (p. 196), was assuredly adrift to the right; the German Democratic party, too, which had had such an important hand in the charting of the constitution, was increasingly deserted by champions of the democratic Left and staffed, instead, by rightists; and the German People's party was in staunch support of the princes under the leadership of Gustav Stresemann, who, while following an enlightened course as the republic's foreign minister, always remained a monarchist at heart.

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STEFAN MARTENS. *Hermann Göring: "Erster Paladin des Führers" und "Zweiter Mann im Reich."* (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Schöningh. 1985. Pp. 405. DM 48.

Scholars should pause before rejecting a dissertation written under an authority such as Klaus Hildebrand and should show due respect for a book whose 238 pages are buttressed by 138 pages of footnotes and sources. Examination is necessary of revisionist views, even of a recognized demon, well documented in R. J. Overy's *Goering* (1984). Attacking the consensus of a half century, Stefan Martens carefully chooses his battlefield—prewar diplomatic history and domestic power struggles—and draws ammunition from British documents. Thus narrowing his vision, he concludes that Hermann Goering was a moderate, working for peace with Britain.

After so many blood-filled books about Nazis, Martens's remarkably bloodless account is an achievement. He notes Nazi violence in passing: Goering criticized Goebbels's *Krystallnacht* and scolded Himmler's nastiness in Poland. In Martens's scenario Goering, after surrendering the Gestapo, left the police league to play in the more genteel businessman's league with Hjalmar Schacht. Goering really wanted to be foreign minister and was frustrated when Hitler chose Joachim Ribbentrop for the post. Martens leaves the impression that an amiable Goering was popular except with Nazi radicals and omits a judgment made by Mussolini: "the ex-inmate of a lunatic asylum; not conspicuous for reason or logic" (Overy, p. 32).

Although appointed Hitler's "successor" in 1939, Goering was being pushed aside by the Führer, who thought him insufficiently belligerent. He bristled at being brusquely called back from vacation during the Prague crisis and was stung when Hitler called him a coward, "like the other generals." Pressed to prove his courage, he warned still in August against going *va banque*, to which Hitler responded that that was how he had always played the game (p. 196). Goering lamented that his opinion carried no more weight with Hitler than a piece of gravel under the Führer's foot. (The consensus is that a foolish Goering was disregarded only after Stalingrad.)

Support for Martens's minority view derives from Goering's words, from contemporaries' judgments, including Chamberlain's, and from various neutrals' efforts to negotiate with him to avert the war. Martens weakens his argument by including Goering's words to Mussolini: "Our motto should be to talk of peace but to prepare for war and victory" (p. 176). The reader might understandably ignore Goering's talk of peace and look at his preparations for war. Goering might have shared the generals' belief that Germany needed more years to prepare, so that his "peace in our time" was more pragmatic than principled.

Martens immediately concedes that Goering's personality is beyond his understanding and avoids analysis deeper than a fear of war and Hitler. More credible would have been a multidimensional Goering, who could bully as well as cringe, and acquire, if not coordinate, an economic empire.

The portrayal of any human as pure devil is flawed. Goering might well have been less irrational than the man to whom he irrationally sold his soul, and perhaps he behaved more evilly to maintain his master's trust, yet he helped Hitler achieve and mightily abuse power. Any well-concealed efforts at moderation were manifestly unsuccessful.

E. N. PETERSON
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ULRICH HERBERT. *Fremdarbeiter: Politik und Praxis des "Ausländer-Einsatzes" in der Kriegswirtschaft des Dritten Reiches.* Bonn: J. H. W. Dietz. 1985. Pp. 494. DM 38.

The foreign labor program of Nazi Germany has received considerable attention in the past fifteen years. Western scholars have been intrigued by its ideological, economic, and political aspects, whereas Eastern scholars have pressed their case linking the program to capitalism. Finally, a young West German scholar has written a comprehensive study using the best of the work of both camps. Drawing from the rich load of archival materials and secondary works now available, Ulrich Herbert has written simply the best account of the program to appear yet in print.

Herbert argues that the program evolved as a response to Nazi ideology and the circumstances of the war. No predetermined plan existed for the mass use of foreign labor, indeed Nazi thinking opposed it, but the possibility of exploiting foreigners was latent in Nazi racism. The Nazis were always wary about such a program, troubled about the German public's reaction to it and its racial effects, and skeptical of its economic benefits. Only the acute shortages of labor in agriculture and industry after the Polish campaign convinced them to improvise a program but a program that would not compromise their racial tenets. Poland became a model to which Nazi leaders cautiously tested German reaction; finding little resentment, they pressed on. Gradually the whole paraphernalia of racial and legal restrictions were placed on the foreigners, while the police, encroaching on existing legal authorities, gained total control over them. By the fall of 1941 the system was in place and functioning rather well. Although adjustments were later made in the program, its basic racial character was never

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seriously altered. Herbert is at his best in describing the tensions that existed between Nazi ideology and practice and how they never were resolved. Even after Stalingrad and Goebbels's propaganda offensive calling for a united front against bolshevism, the hard-liners refused to make significant concessions to the foreigners. The results were predictable—including the terrible bloodbath against the foreign workers at the end of the war.

The economic importance of the program can hardly be denied. By August 1944 7,615,970 foreign workers were employed in the Greater Reich. Nearly half of the workers in agriculture and the armament plants were foreigners, and in the metal, chemical, construction, and mining industries they represented a third of the work force. Despite their mishandling, they were productive workers, expensive to use and expensive in terms of Nazi occupational policies and goals but indispensable for the war economy. More alarming was the ease with which the German public and industrialists accommodated themselves to the harsh racial policies of the Nazis.

This work has all of the earmarks of solid German scholarship—careful research, balanced analysis, and able writing. The author and publisher can be proud of their work.

EDWARD L. HOMZE
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KATHARINE PARK. *Doctors and Medicine in Early Renaissance Florence*. Princeton: Princeton University Press. 1985. Pp. viii, 298. $40.00.

This book is a richly detailed study of Florentine medical practice during the fourteenth and fifteenth centuries. Katharine Park traces the early professionalization of medicine among educated physicians and the attempts of that professional elite to shape medical services in Florence. This work will serve well as a model for histories of medical personnel elsewhere in early modern Europe, for, unlike many other surveys, it is not a catalogue or biographical dictionary of practitioners. Park's study is brimming with archival detail of the careers, prospects, and ambitions of doctors, which she embeds within the culture of the early Florentine Renaissance.

Comparative studies will be necessary because Park's conclusions go beyond what can be demonstrated with certainty from Florentine records. Park argues that the recurrent plagues of 1350–1450 reduced the prestige and desirability of a career in medicine, particularly among Florentine families who could hope for access to political power through membership in the medical guild. Florentine physicians who survived the first few devastating epidemics in the fourteenth century tried to reverse their crumbling social status by creating first a university and then a *Collegio* for educated practitioners. Younger Florentines were not persuaded to enter the guild, and consequently immigrant doctors—many of them quacks, barbers, or other low-status healers, but quite a few established physicians from smaller subject towns as well—flooded the Florentine medical marketplace. Medicine could not be a stepping stone to political power after 1400.

At the same time Park shows brilliantly the opportunities for material rewards for those who chose a medical career after the plague, the educated physicians' growing concern with the nobility of medical service, and the eventual victory of schooled physicians over their rivals in the guild of physicians, grocers, and apothecaries. Park also concludes that Florence in the early Renaissance was culturally and materially a magnet and that medical personnel were one of the many groups it attracted. We must therefore believe that, although wealthy Florentine families were reluctant to encourage medical careers among their own progeny, even when high financial return was likely, elsewhere this was not the case (or elsewhere the plague did not have the same effects on career choices). Perhaps one instead became a doctor to emigrate, to "see the world." We thus need comparative studies of quality equal to Park's to analyze effects of plague on the professionalization of medicine, for the fact that many physicians were immigrants to Florence ensured that they would be political cyphers and of lesser social standing, not that impotence against plague had left their calling undignified.

Park gathers impressive documentation to describe the careers and affiliations of medical men in Florence. As they turned away from aspirations of political influence during the fifteenth century, doctors were constantly torn between the mundane medical tasks that led to financial security (*utile*) and the intellectual and spiritual pursuits that would make physicians a vital part of the Florentine Renaissance (*honore*). Park does not accept slavishly the old model of humanist/lawyer opposed by scholastic/physician and thus can quickly find exceptional cases and show sensibly that, as is often the case today, physicians were simply too busy as practitioners to participate actively in humanistic ventures.

Many readers will find the appendixes to the book useful: one summarizes names, dates, places, and money in using Florentine sources, another enumerates the lecture schedules and topics typical in university medical education, still another gives a careful guide to manuscripts of doctors' letters. A fourth lists all the practitioners included in the Florentine fiscal survey, the *catasto*, of 1427. It shows that over a third of the physicians with any wealth at

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all had survived the great plague of 1400, yet another third clearly entered the profession before the plague of 1417. Physicians may have been seen as useless and greedy in the face of epidemics, but they apparently were not in certain danger of losing their lives.

Park succeeds well in showing that the organization and professionalization of medical services and medical personnel responded dynamically to changes in urban cultures of early modern Europe, and reading this book deepens one's understanding of the process of change. Although some of the overarching conclusions of the book can be questioned, the research that went into its production sparkles.

ANN G. CARMICHAEL
Indiana University

CLAUDE V. PALISCA. *Humanism in Italian Renaissance Musical Thought*. New Haven: Yale University Press. 1985. Pp. xiii, 471. $40.00.

If any familiar historical term is under perpetual threat of loss of meaning, it is "humanism." Accordingly, it is a pleasure to read this new book by Claude V. Palisca, which contributes substantially to the reconstruction of the term in its primary historical significance by connecting humanism to the theory and philosophy of music in the Italian Renaissance. The book not only presents much new material but also gives us a distillation of Palisca's many years of experience as a major scholar in the field of Italian musical thought of the Renaissance and early baroque.

From its title one might suppose that the book provides a wide-ranging survey of the main lines of its subject. In fact, it does that to a considerable extent, though always within the perimeter of music theory and aesthetics, not with reference to the specific impact of humanism on musical styles and compositional procedures. But Palisca did not rest content with giving us an improved guide to the familiar; he adds to the general topics of the book an examination of little-known aspects of the field, including discussion of writers whose roles in the development of musical humanism have been obscure. This results in a text of considerable density, yet one still accessible to musicologists and intellectual historians. The book will repay the close reading that it requires, and it is likely to hold a position of leadership in its field for many years to come.

After an opening chapter that sets the historical background and the limits of the study, Palisca develops the familiar but essential theme of the rediscovery and translation of the basic texts on music from antiquity and discusses the importance of these texts for many of the major Italian writers on music in the fifteenth and sixteenth centuries. He reviews the ideas of the earliest writers he would admit to the company of musical humanists, including Pietro d'Abano, Giorgio Valla, and the little-known Carlo Valgulio, whose Latin translation of Plutarch's *De musica*, published in 1507, is given its first modern exegesis. Moving into the sixteenth century, he deals at some length with the important translator Antonio Gogava (d. 1569), heretofore known only as a figure in the circle of Zarlino. Thereafter, Palisca devotes a series of important chapters to major issues influenced by humanistic traditions that occupied Renaissance theorists: the problem of the harmony of the spheres, the connection between ancient *musica speculativa* and Renaissance musical science, the celebrated problem of the ancient Greek *tonoi* and octave species in relation to Renaissance mode, the role of grammar and accent in Italian theory, and, finally, the poetics of music and dramatic theory in relation to music.

Only the first chapter disappoints to some extent. Palisca restates the traditional problem of why Italy, the seedbed of humanism, was not equally the place of origin of the basic musical styles of the Renaissance; as he puts it, the paradox is that "much of the written music exhibits style elements of undeniably transalpine origin" (p. 5). But, as he knows, much of the music of some of the most famous *oltremontani*, certainly including Josquin, was actually composed in or for Italy and its patrons. Furthermore, beginning with Josquin clear signs indicate the significant effect of the tenets of humanism on style, at least in certain classes of works. To have dealt with this would have gone far beyond the purpose of this book, which admirably fulfills its aims while remaining within the domain of formal musical theory and speculative thought. The book as it stands is a major contribution to the history and broad intellectual background of music theory, and it will be of value to a wide range of readers in both music history and the development of Italian humanism.

LEWIS LOCKWOOD
Harvard University

PIETRO REDONDI. *Galilée hérétique*. Translated from the Italian by MONIQUE AYMARD. (Bibliothèque des Histoires.) Paris: Gallimard. 1985. Pp. 447. 150 fr.

Pietro Redondi's book is historiographically interesting, for it challenges the normative view that Galileo was condemned in 1633 for his defense of the Copernican theory in the *Dialogue on the Two Great World Systems*. Redondi argues that Galileo's condemnation had to do with his treatise *Il Saggiatore*. According to Redondi, the Jesuit Orazio Grassi wrote a delation to the Inquisition that argued that *Saggiatore*'s atomism threatened to undermine the

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doctrine of transubstantiation. If Redondi's argument is valid, then our understanding of the Galileo affair must be seriously modified. In principle, nothing is wrong with revisionism, although one hopes that the evidence for revision is strong enough. This may be the problem with Redondi's book.

Written in a breathless, journalistic style, the book is both spellbinding and troublesome. One Italian critic—Sergio Pagano, editor of *I documenti del processo di Galileo Galilei* (1984)—has persuasively argued that the document supposedly penned by Grassi ("G 3") is not in Grassi's hand. Although Redondi's book is richly footnoted, one worries, first of all, about an argument that rests on a document not written by the putative author (Grassi). In addition, this document, which Redondi would have us think was a delation, does not follow the form of a delation; for example, it does not have an attestation of the delator's own spiritual status and comportment. If the document is not a delation, then its standing in the proceedings was very different than Redondi would have it. It is more likely that this document was written by an employee of the Inquisition who perused writings of the accused to see what could be found. This characterization of the document is preferable, especially since Redondi's description of it as a denunciation to the Inquisition (p. 177) is not supported by a reading of "G 3"'s words; for example, there is no mention of a fortuitous reading done without preconceptions. And, since the charge of atomism has no place in the final judgment against Galileo, the fathers inquisitor clearly did not care to take into account the argument found in "G 3."

The French translation differs from the original Italian in that it contains a postscript by the author, in which he responds to his Italian critics (Pagano and Mario d'Addio, in particular). Conceding that "G 3" may not be in Grassi's hand, Redondi does not see that this reduces its status to an inquisitorial in-house investigative piece that might have, but did not, open up the atomism issue. Instead, Redondi counters with a suggestion by innuendo of mid-1980s conspiracy: he attacks Pagano and d'Addio because they write in Vatican publications. But to attack the reliability of their scholarship simply because of their physical propinquity to the papal apartments is a bit hard to credit—at least without supporting evidence.

Redondi's book will continue to stir up controversy. It is unfortunate, however, that he has sidetracked the scholarly argument with this sort of *ad hominem* attack.

EDWARD A. GOSSELIN
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JOHN A. PINTO. *The Trevi Fountain*. New Haven: Yale University Press. 1986. Pp. xviii, 323. $30.00.

John A. Pinto's book is a successful effort to integrate architectural history into urban history. In Rome the papacy, city government, and families of the elite influenced designs for this particular fountain, the most famous in the city today. Using a variety of sources, Pinto traces the history of the fountain from antiquity until the eighteenth century, when Nicola Salvi won a commission to design the fountain as we know it. Pinto has brought to light numerous unexecuted designs for the fountain from the Renaissance and baroque periods, many in collections outside Italy. His text provides a definitive commentary about these designs and extends beyond matters of authorship, dating, and patronage into aesthetic criticism. He combines analysis of particular designs with the social and political strategies of popes and Roman families so that the architectural historian's concern for tracing the influence of one artist on another never comes at the expense of the social historian's concern for the interests of the elite. Because water was supplied in public spaces, the design of fountains provided an opportunity for architects to promote political order and advance the interests of the elite. The publisher should be complimented for the quality of reproduction of so many drawings of Juvarra, Fontana, Bernini, and Borromini, and by others whose names are less well known. The dimensions of the originals are not given, and catalogue numbers for only a few drawings are provided.

In 1730 the Conti family completed a new wing of their palace facing the unembellished fountain at the Trevi. In the same year the newly elected pope, Clement XII, initiated a competition for a fountain, which had to incorporate the new wing of the Conti palace. Nicola Salvi won the competition with a design that is his only masterpiece. Pinto, who devotes a chapter to the history of the construction of the Trevi (1732–62), takes great care to explain the technical and conceptual problems the artist had to solve through his design. Other artists wanted sculptural pieces to dominate the design, but Pinto explains how Salvi fused "architecture with sculpture and water into a monomorphic unity" (p. 121). In the process Pinto uses Salvi's manuscript description of iconography to explain how the design transformed a traditional image of water as a symbol of destructive nature into a symbol of energy and benevolence appropriate for the Enlightenment. The book ends with a chapter on the place of the Trevi Fountain in the history of aesthetic criticism.

Pinto is to be commended for undertaking such an ambitious study, which sets architectural design into cultural and social contexts. But the contexts are of the elite only. If the book has any major

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shortcoming, it is the omission of material on the popular use of fountains in everyday life in Rome. This topic might have encouraged Pinto to discuss more extensively the role of fountain designs in urban planning in the nineteenth and twentieth centuries.

JOSEF W. KONVITZ
Michigan State University

TIMOTHY DAVIES. *Famiglie feudali siciliane patrimoni redditi investimenti tra '500 e '600*. Foreword by STUART WOOLF. (Unione delle Camere di Commercio Industria Artigianato ed Agricoltura della Regione Siciliana, Storia Economica di Sicilia, Testi e Ricerche, number 27.) Rome: Salvatore Sciascia. 1985. Pp. 236. L. 20,000.

As Stuart Woolf notes in the foreword to this book, Timothy Davies's topic is important and his research in local archives extensive. Yet the result is strangely disappointing, largely because it lacks an adequate framework for the data painstakingly gathered in the Sicilian archives. As Davies himself concedes, this work is a slightly modified version of part of his dissertation, and it has the solid virtues and the serious flaws of many a dissertation core. The research is indeed extensive, for Davies traveled to numerous Sicilian archives and delved deeply into state and private papers, and the quantitative base for the work is impressive.

But the book is little more than four case studies dealing with the five families whose patrimonies and whose investment and marriage strategies Davies dissects. The book lacks an interpretive framework except for Woolf's lucid but brief introduction, which sets the work in the context of studies on the aristocracy. The conclusion does offer some interesting comments, but they are compressed in three pages. The families discussed exemplify the broad range of responses to familial and wider economic pressures; the fate of their patrimonies and positions (eroded or destroyed for two; strengthened and consolidated for the others) reflects the dangers and the opportunities for aristocratic families in Sicily, as elsewhere in the Mediterranean, in the sixteenth and seventeenth centuries. Lamentably, though, the study has no thesis and, until the conclusion, almost no sense of direction; the reader must create the patterns from an episodic narrative and establish the connections between, and even within, each case study. The work appropriately straddles economic and social history and is strong on agrarian developments, but nowhere does it provide a discussion of the structures and trends of the Sicilian economy. These flaws are all the more puzzling because Davies has published an exemplary article on themes discussed here in the *Journal of European Economic History* (43 [1983]: 371–405).

I have one last but important criticism. This is a work on Sicily in the sixteenth and seventeenth centuries, a time when the island, like Naples and Milan, was a Spanish possession. But this is a work written without the benefit of the rich Spanish documentation, particularly Simancas's *Estado, Visitas de Italia*, and the *Secretarías Provinciales* series. And yet it deals with issues that those documents could have placed in proper perspective or enriched—population growth and the export of grain, the profiteering of state officials and the purchase of noble titles, to mention only a few. It is impossible or unwise to write on Spanish Italy without doing research in both Spain and Italy. That is a categorical lesson, but not a novel one. One wonders why it was lost on Davies.

ANTONIO CALABRIA
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MARCO CATTINI. *I contadini di San Felice: Metamorfosi di un mondo rurale nell'Emilia dell'età moderna*. (Fondazione Luigi Einaudi Studi, number 3.) Turin: Giulio Einaudi. 1984. Pp. xvii, 364. L. 30,000.

Located in the northern part of the provence of Modena, which was once a duchy, the commune of San Felice has a long history, and Marco Cattini has produced a dense, complex, first-rate study of its decisive period, roughly from the latter part of the sixteenth to the early eighteenth century. He has divided his book into five chapters, each dealing with a separate but interrelated topic: environment, demography, economy, social structure, and, finally, "Un tentativo di sintesi," which is not really a synthesis, but an analysis of the loss of cohesion among the local population and the loss of autonomy by the communal administration.

Cattini's methods are dynamic in that he studies the evolution of these aspects of history over roughly two centuries; his study is quantitative when he has reliable data and theoretical when he uses San Felice as a case study to test several theories put forward by economists, sociologists, anthropologists, and historians. He is well trained in economics and widely read in these other fields. His book is a microhistory, but one with broad implications. He knows the province well, having published a book on it, and he has uncovered a vast number of sources, mainly archival, that form a solid basis for his conclusions.

The commune reached an agricultural golden age from about the 1570s to 1620. The environment underwent dramatic changes as technical progress

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made headway. In fact, Cattini refers to a "small agricultural revolution." If I understand him correctly, this technical progress refers not to newly invented machinery but to the transformation of land use: more effective irrigation, expansion of arable, decline of pasturage and forest, and strips of land combined into medium and small holdings. Useful trees such as mulberries, elms, and poplars were planted along the edges of fields of cereal crops, and vines were attached to them. Cultivation became more intensive to feed a growing population. Ultimately, however, peasants did not improve their living standards and even suffered a decline in the quality of their diet after abandoning the raising of food animals.

The part on demography reveals that San Felice's population differed little from the rise of Europe in early modern times. More interesting is his testing of the economic theory of exchange. The peasants' main aim was autarkic production, to grow sufficient grain to feed a family and finish with a surplus to be used as seed-grain in the following year. A surplus for sale in a distant market was of less concern. The market was a mystery. Cattini found that the quantity of grain had an inverse correlation to price: higher prices resulted from small harvests. But high prices did not bring higher monetary incomes. Cattini argues that historians, when studying a preindustrial economy, can fall into error if inspired by theoretical models of a market economy.

From the 1630s onward, general conditions worsened. Indebted farmers lost much of their land to outsiders, to nobles and bourgeois from Modena and Ferrara. The number of landless workers rose. Nonresident owners undermined the taxing and administrative powers of the communal council, which had controlled local families that were really clans. The golden age had long passed and decline set in, at least for the locals. An early form of capitalist agriculture replaced the old autarkic policulture, and the traditional collective sense of community vanished as divisiveness set in. Concentration of land in fewer hands did not lead to modernization but to "schlerosis of the economic and social structure" (p. 148). The "little agricultural revolution" was short-lived for most of the natives. Incidentally, the natives were not all true peasants; rather, *contadini* is best translated as "countrymen," because some of the population would more surely fall into the category of gentry, especially during the golden age.

LEO LOUBÈRE
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NELLI-ELENA VANZAN MARCHINI, editor. *La memoria della salute: Venezia e il suo ospedale dal XVI al XX secolo.* Venice: Arsenale, 1985. Pp. 285.

Through reorganization and development the historic buildings that have long housed the civic hospital of Venice are being released for new uses. Among them are the Scuola Grande di San Marco, well known for its magnificent fifteenth-century trompe-l'oeil façade; parts of the adjoining monastery of SS. Giovanni e Paolo; and the hospital of the Mendicanti. An imaginative proposal for the use of these areas includes the establishment of an archive and a permanent exhibition on medicine and health. It is hoped that the hospital's rich patrimony of art, architecture, books, documents, and scientific instruments can thus be made available to the people of Venice.

La memoria della salute is the title of the exhibition that launched the project in 1985 and of its catalogue that is reviewed here. The exhibition focused on the buildings that form the present hospital complex and on the institutions that formerly occupied them. These were primarily the Scuola di San Marco, a charitable guild of great religious and social significance, and the hospital of the Mendicanti, a medieval leper hospital reestablished in the late sixteenth century principally to house the destitute and to eliminate begging. Other common ancestors of the civic hospital that were originally located elsewhere in the city (notably, the hospitals of the Derelitti and the Incurabili) were not dealt with in the exhibition or its catalogue. This is surprising in that, at least before the eighteenth century, their medical functions were more important than those of San Marco or the Mendicanti. The exhibition, however, reminded us that, until recent times, hospitals served a variety of religious, educational, and social functions of which medical relief was only one, and the catalogue is, therefore, certainly of interest to a wider readership than historians of medicine. A stimulating article by Brian Pullan, for instance, concentrates on San Marco and the Mendicanti as pillars of the social structure, for they defended morality and public order in Venice and eased the city's poor through the subsistence crises that threatened social stability. Further articles discuss the contribution of these institutions to the history of art and, through the choir school attached to the Mendicanti orphanage, to the history of music. From the eighteenth century onward, the medical theme gains greater prominence. A valuable essay by Nelli-Elena Vanzan Marchini, principal organizer of the exhibition, breaks new ground by tracing the development of the hospital from the closing years of the republic through the reforms of the Napoleonic and Austrian regimes to the twentieth century. Additional articles cover surgical instruments and medical photography and illustration, although not always with sufficient reference to the context of the Venetian hospital.

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The catalogue is beautifully produced and well illustrated, an important feature since it brings together photographs of the hospital's art collection (including works by Bellini, Tintoretto, and others), which is now widely dispersed. It also presents photographs of the historic collection of surgical instruments, and some stunning examples of early twentieth-century clinical photography. The exhibition certainly deserves to become a permanent feature of the cultural life of Venice.

RICHARD PALMER
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GIULIO CIANFEROTTI. *Giuristi e mondo accademico di fronte all'impresa di Tripoli.* (Quaderni de "Studi Senesi," number 56.) Milan: Dott. A. Giuffrè. 1984. Pp. viii, 167. L. 15,000.

As Giulio Cianferotti tells us, this little book was conceived as part of a larger study, now close to completion, about the attitudes of Italian academics toward the Great War. The section on the attitudes of the Italian legal and academic community toward the Libyan war could stand on its own, Cianferotti decided. With a more analytical treatment or a clearer line of argument, perhaps it might have. As it stands now, however, the book is little more than a compilation of quotations from the learned world of the day and can serve as a convenient source. As a synthesis or analysis of the problem, the book is useless. Moreover, if Cianferotti continues in the same vein in his larger study, the result will be equally disappointing.

Cianferotti's lack of analysis is regrettable, for his material raises many interesting problems. At the outset he hints at a fascinating problem: a linguistic analysis of the rhetoric of the Libyan war. No matter what the discipline—juridical or economic, philosophical or literary, historical or archaeological, botanical or geological—he sees a uniformity of language, imagery, and themes that permeate the discourse (p. 2). He fails to develop his argument, however, and only compiles speeches before learned societies, rectoral addresses, newspaper articles, and the like.

He divides his material into four chapters: "The Resurrection of Italy," "War and Law," "Academic Congresses during the 'Libyan Years,'" and "The Moral Value of War." As the chapter titles indicate, the author does not pursue a clear line of investigation: three chapters seem to favor a thematic approach, but the fourth is grouped around institutions (academic congresses). Nor does Cianferotti explain the criteria for his selection. Why does he include this address and not that one? Some of the papers at legal congresses, for instance, seem irrelevant to the issue of the Libyan war (p. 65).

Here and there the author alludes to another interesting problem: the relationship between the reaction to the Libyan war and the intervention crisis of May 1915, barely two years later. From time to time he also drops hints. In February 1913 students at the University of Genoa, for instance, invited their colleagues from other universities to participate in a ceremony at Quarto—"almost an anticipation of the 'Sagra di Quarto del maggio radioso,'" writes Cianferotti (p. 38), but he then drops the matter.

Nevertheless, the book is valuable simply as a source, a detailed guide into the academic and legal world of the era of the Libyan war. In his extensive footnotes, which on certain pages threaten to blot out the text, the author provides a mine of information. Who succeeded whom to what chair? Whose contribution was well received at which congress? What was the student enrollment in each faculty at the various Italian universities, and what were their budgets?

In short, this is potentially a valuable mine, which only awaits miners to dig into the questions that Cianferotti raises but never answers.

CLAUDIO G. SEGRÈ
University of Texas

JOSIP ADAMČEK *et al.*, editors. *Seljačke bune u hrvatskoj u XVII stoljeću (Gradja)* [The Croatian Peasant Revolts in the Seventeenth Century: Documents]. (Izvori za Hrvatsku Povijest, number 6.) Zagreb: Centar za Povijesne Znanosti Sveučilišta u Zagrebu. 1985. Pp. xv, 646.

In recent years Croatian historians have directed a great deal of attention to the study of peasant unrest in the Croatian lands during the seventeenth century. They are primarily interested in ascertaining the causes and results of the peasant disturbances and in publishing primary sources on the subject. One Croatian historian who has been particularly active in this field is Josip Adamček, the principal compiler of the collection of documents reviewed here. The publication of documents on the seventeenth-century peasant disturbances was jointly initiated in 1973 by the Center for Historical Sciences and the Archive of Croatia. In the next few years a number of important documents were published in the *Arhivski vjesnik* (vols. 16–20). Finally, Adamček and his collaborators have come out with a 646-page volume that, together with the documents already published, constitutes the largest collection of source material on the peasant disturbances of the seventeenth century. Nearly all of the sources are in the original Latin, and only a few are in

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Serbo-Croatian. The documents are located in four of Croatia's major archives and in the Hungarian State Archive in Budapest. Much more remains to be done before all the sources, unpublished and published, on the seventeenth-century peasant unrest are brought together into a single collection.

In the course of his research Adamček uncovered documents on the peasant uprisings that were previously unknown. This has prompted him to initiate a more systematic study (now in progress) of the seventeenth-century peasant unrest than has hitherto been possible. In the meantime, the collection of documents that he has published will be of value to historians in Yugoslavia as well as to those abroad. It will interest not only those concerned with peasant uprisings and rural unrest but also those concerned with Croatian history in general, because, after all, the peasant uprisings cannot be separated from the political, social, and religious issues of the time.

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Österreich im Europa der Aufklärung: Kontinuität und Zäsur in Europa zur Zeit Maria Theresas und Josephs II. In two volumes. (Internationales Symposium in Wien, 1980.) Vienna: Österreichische Akademie der Wissenschaften. 1985. Pp. 621; 624–1102. S 520.

These two volumes discuss three general categories of themes on the subject of continuity and departure in the reigns of Maria Theresa and Joseph II: economic and social questions; the state and the law; and art, literature, and education. Since it would be impossible to address, even briefly, each of the fifty-two contributions in the space available to me, I shall confine myself to discussing those that appear to me to have the greatest general interest.

In her keynote paper Grete Klingenstein sees Joseph's rule essentially as a response to an enduring crisis. His reforms were not part of a systematic program but rather *ad hoc* reactions to rapid changes in the power struggle between the monarchy and the great landholders as represented in the estates.

The late Robert Kann argues that the effect of the physiocratic reforms implemented under Maria Theresa was not so much an improvement of Austria's economy, which continued to be backward, but rather a series of advances in human rights. These reforms, though unspectacular, were so solidly grounded that, in essence, they survived several decades of reaction. Kann concludes that Maria Theresa's foreign policy went against tradition, since, unlike her immediate predecessors, she strove not to acquire additional territories but merely to defend the existing realm.

Ernst Wangermann maintains that Austrian history after 1740 represents, in the main, a continuum. But, because of Maria Theresa's essential timidity, the reform ideas of Wenzel von Kaunitz and Joseph von Sonnenfels were only implemented in part. Joseph did not have a program of his own but merely insisted on the more consequent implementation of the reforms already planned in his mother's reign. His agrarian reforms, however, which struck at the very citadel of landed privilege, precipitated a general crisis. Ultimately, the failure to execute the reform resulted not from a generalized horror at the bloody events of the French revolution, as is generally asserted, but from a shift to a more conservative course, which can be discerned as early as 1784–85. Wangermann sees this as following largely from the Austrian bourgeoisie's rejection of royal absolutism as a proper instrument for achieving social reform, an attitude that paradoxically abetted the aristocratic reaction at the end of Joseph's reign.

Helmuth Feigl argues that it is naive to assume that West European peasants were better off economically than the robot-performing peasants in the East, for the latter had the advantage of being independent of the fluctuations of a market economy. A period of major agricultural crisis occurred during the reign of Maria Theresa because a rapidly rising population put pressure on an agricultural system that, subjected to a major climatic change, produced increasing numbers of harvest failures. Moreover, the expense of waging major wars made increased tax revenue essential. The entire economy, therefore, had to be reformed. Both Maria Theresa and Joseph were convinced that so major a change was possible only through the abolition of the *Gutswirtschaft*, since robot was a notorious school for idlers. Those measures that survived the aristocratic reaction resulted in the creation of a rural proletariat that might well have led, eventually, to the bolshevization of Austria had it not been for the embittered resistance of the Austrian smallholder class, whose position was enhanced and secured by the Theresian and Josephinian reforms.

Arnost Klimá discusses what he terms the weakening of Bohemian feudal structures as a result of the economic needs of proto-industrial enterprise. Economic necessity often forced the owners of such enterprises to make decisions that could be implemented only in a capitalistic, not a feudalistic system.

Gustav Otruba maintains that Maria Theresa's most acute crisis was fiscal. The state debt surpassed 100 million florins, and annual revenue did not rise above one-fifth of this. Even the Haugwitzian reform of 1749, which proceeded to tax dominical lands, at best doubled state income. Kaunitz's fa-

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mous overhaul of the Haugwitz system, largely the work of his protege Ludwig Zinzendorf, was in practice subverted by an aristocratic faction grouped around Friedrich Hatzfeld. Joseph, confronted by an economy left in a state of chaos by half-implemented measures, could not proceed systematically but was forced to improvise.

Helen Liebel-Weckowicz maintains that, in practice, Maria Theresa was more receptive to enlightened ideas than Joseph, who tended to side with the conservatives among his ministers. When it came down to the crunch, he refused to push through the basic reform plans formulated by Karl Zinzendorf and by Kaunitz. The Josephinian reforms that were effective owed their success to the *Kreisämter*, which functioned independently of the local administrative structure controlled by the feudal nobility. Although Joseph relied heavily on the *Kreishauptleute*, his insistence on using systematic surveillance reduced the effectiveness and undermined the morale of the most devoted state employees.

Peter Hersche attempts a comparison of Austrian and Swiss developments in the second half of the eighteenth century. He detects some similarities, but concludes that, because the Swiss emphasized change from below rather than from above, the result, unlike in Austria, was the creation of a substantially radicalized infrastructure closely identified with its actions and finally able to mount a successful revolution.

Jean Béranger lists the essential elements in the centralization of Austria's administration: the Haugwitzian reform, which ended Bohemia's virtual fiscal independence and replaced negotiations with the estates over tax bills with simple promulgation; and the recognition in Vienna of the necessity of granting Hungary a special status. The control exercised in the Hungarian countryside by the *Komitate* was not to be broken by administrative fiat; it would have to be tolerated.

Karl Otmar von Aretin develops the argument that in the re-Catholicized states of Central Europe a close cooperation evolved between church and state, centered on the Marian cult. Whenever the church found concessions to state power necessary, the extent and nature of these were determined by Rome, unlike in France, where the Gallican church was largely subservient to the state. In the early eighteenth century Rome appeared ready to give way to the liberalizing tendencies of the age, but these hopes were shown to be illusory in the pontificate of Benedict XV. The resulting tendency of liberals in the church to look to secular rulers, and in particular to the empire, as a counterweight to the pope established the framework for the great ecclesiastical reforms of the second half of the century. The ecclesiastical innovations introduced by Joseph, however, were carried out in a spirit far from the Jansenist "love for the church" and hence were met with embittered opposition not merely by the ultra-Catholic elements in the Austrian church but also by ecclesiastics who feared for the welfare of the church as a whole. Thus, Joseph created a revolutionary situation in Austria by willfully destroying the bases on which reform could successfully have been implemented in an ultra-Catholic country.

Franz Szábo maintains that, contrary to what is frequently asserted, Kaunitz in no way favored the Austrian Jansenists or their program for reforming the church. Kaunitz always counselled moderation. The anti-clericalism attributed to him surfaced only with the excommunication of Duke Ferdinand of Parma in 1768, and, even then, he favored reforms based not on Jansenist ideas but on enlightened doctrine and secularism.

Arguing that Joseph has been unfairly accused of pursuing aggressive policies bent on the acquisition of territory, Derek Beales favors a general revision in the assessment of Joseph's foreign policy goals. In fact, it was Kaunitz who usually recommended aggression. Joseph himself tended to give primary consideration to internal political questions, and, as his military performance showed repeatedly, inclined to extreme caution. In the actual conduct of foreign affairs, he would have preferred, as did his brother Leopold, to accommodate the Prussians.

Addressing himself to Joseph's aesthetic side, Gerhard Croll points out that the *caesura* in the history of music represented by Haydn's and Mozart's chamber music of the 1780s undoubtedly escaped Joseph's notice. The emperor's musical tastes were in large measure dictated by the violinist Franz Kreibich—a virtuoso, to be sure, but a man whose judgment was otherwise so impaired that he played a role not dissimilar to that of court fool in the emperor's entourage—and by *Kammerdiener* Johann Strack, who played the cello in the amateur string quartet in which Joseph occasionally performed and of whose good common sense Joseph entertained a high opinion.

Notker Hammerstein points out that the great changes introduced in the Austrian university system were not the result merely of Gerhard van Swieten's efforts but also of a conformance to the reappearance, for the first time since the Reformation, of a consciousness of the universality of knowledge. That these changes manifested themselves in Austria relatively late in the course of the European Enlightenment was the result of a paradoxical development; the imperial monarchy, as it expanded its power in the seventeenth century, had experienced a continual need for trained and knowledgeable bureaucrats. Particularly after 1648, these were brought in great numbers from the *Reich*, so that a surplus of academics and intellectuals was available

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in Vienna, and thus no pressing need existed to turn them out at home.

Edith Rosenstrauch-Königsberg unconvincingly attempts to derive a feeling of Austrian nationalist consciousness from the protracted struggle against Prussia in Maria Theresa's reign. Sonnenfels's famous *Vaterlandsliebe* may have been in part national patriotism along the lines of the Roman model so popular at the time, but just as likely it was nothing more than the *Kaisertreue* of the outsider—in his case, of the converted Jew. Speculations about possible connections between Austrian Freemasons and *Illuminati* and those who opposed Joseph's unsuccessful attempt to introduce German as the language of the administration in the whole of the monarchy do nothing to advance the argument.

James Van Horn Melton examines the symbolic and sacramental aspects of the monarchy in terms derived from Foucault. These tended to take the form of *pietas austriaca*; pilgrimages, religious processions, and the cult of local saints were particularly prominent. In return for Habsburg support, the Austrian church was in the forefront of the movement to exalt imperial absolutism. This symbiosis found a further expression in the school plays that were a part of the curriculum everywhere and were invested—principally by the Jesuits—with the didactic function of inspiring respect and veneration in the young. In contrast to this, theater in Austria, with the advent of Joseph Stranitzky and the *Hanswurst*, played a central role in the intellectual legitimization of the nonnoble population through the emphasis of secondary plots, which took up as much stage time as the primary ones dealing with traditional aristocratic themes.

In a second contribution Wangermann returns to the subject of continuity in eighteenth-century reforms of agriculture, religion, and education. His argument on education appears more convincing than it is because it is based exclusively on Karl von Martini's reform proposal of 1773 and passes over in silence the ambitious reform plan proposed by Johann Baptist Pergen just before Martini's system was accepted.

The extensive bibliography will undoubtedly prove essential to students of the period but is extremely difficult to use because of its organization.

On the whole, these two volumes constitute an impressive body of research. They will doubtless be used as a central reference work for the Theresian and Josephinian period for a long time to come.

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EVA SCHMIDT-HARTMANN. *Thomas G. Masaryk's Realism: Origins of a Czech Political Concept.* (Veröffentlichungen des Collegium Carolinum, number 52.) Munich: R. Oldenbourg. 1984. Pp. 211.

Eva Schmidt-Hartmann's short monograph is among several recently published books that address aspects of the life and thought of T. G. Masaryk. As its subtitle indicates, she emphasizes his development of the concept of "realism" rather than the extent to which it and related ideas conditioned his politics before 1914. Her nine chapters are not well integrated and may be read as independent essays. The first three survey historical and political background that is much more thoroughly discussed in other scholarly works. The fourth perceptively discusses realism in the Austro-German intellectual tradition and compares this "realism" to that of Masaryk without indicating explicitly its influence on him. Chapters 4 and 5 clearly describe the intellectual development of the three leading Realists—Masaryk, Josef Kaizl, and Karel Kramář—and their entrance into politics in the early 1890s. The sixth chapter summarizes Masaryk's understanding and advocacy of "realism" in four of his controversial treatises of the 1890s and concludes with an informed critical assessment of Masaryk's concept of realism. This assessment lacks an equally thorough discussion of his most ambitious work of that decade, *The Social Question*, and of his actions and statements after 1900 that indicate to what extent he came to recognize the inadequacy of some of his earlier views.

In Chapter 7 Schmidt-Hartmann intelligently reviews Masaryk's debates on politics and history with Kaizl, Josef Pekař, and Antonín Hajn. Although many scholars have as thoroughly evaluated Masaryk's differences of opinion with Kaizl and Pekař, Schmidt-Hartmann presents the most extensive commentary to date in English on the relationship between Hajn and Masaryk. Her superficial eighth chapter on realism and its political practice from 1900 to 1914 is so poorly integrated with the other chapters that it appears to have been appended as an afterthought. In this and the ninth chapter on Masaryk's elaboration of realism up to 1914, she makes no references to pertinent scholarly studies by Stanislav Jandik and Roland Hoffmann, does not comment on the indexed, 192-page Realist (Progressive) party program of January 1912, uses an incomplete copy of the first Realist (Ramcový) program of April 1900, takes no account of Masaryk's voluminous correspondence or of criticism of his politics in the Czech press after 1900, and says nothing new about the reality he sought to change or the difficulties he encountered in trying to do so. Furthermore, she says little about Masaryk's growing support for the South Slavs and Slovaks after 1906 or about his advocacy from the early 1890s onward of women's emancipation, al-

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though this support and advocacy arguably are relevant to any understanding of his "realism."

Sometimes evident is the author's limited understanding of Austro-Hungarian constitutional law and government, especially at the provincial, district, and local levels. Unproven, for example, is her assertion that "the non-German nationalities like the Czechs, with their mainly nationalistically-oriented policies, largely prevented parliament from serious, meaningful political work" (p. 115). On the same page she is mistaken in contending that, after discussing the Reichsrat and the Cisleithanian constitution in his parliamentary speech of March 1895, Masaryk did not "ever return to this topic in his later writings." Not only did he and his colleagues repeatedly do so in the 1900 and 1912 platforms of the Realist party, he referred to it in many of his political publications from 1898 to 1914.

Schmidt-Hartmann's book will primarily interest readers seeking an informed discussion of Masaryk's "realism" as reflected in his published works of the 1880s and early 1890s and an intelligent, critical historical survey of that "realism" up to 1914. The value of the book is somewhat diminished by its arbitrary emphasis on realism before 1900 and by its lack of an index.

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GARY W. SHANAFELT. *The Secret Enemy: Austria-Hungary and the German Alliance, 1914–1918.* (East European Monographs, number 187.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. xi, 272. $30.00.

In this short monograph Gary W. Shanafelt traces from the Austrian side the evolution of the Dual Alliance from 1914 to 1918. As he points out, numerous modern studies have examined aspects of the problem, yet none has attempted a comprehensive coverage. To supply this deficiency, the author offers a political and diplomatic assessment of Austrian leaders' motivations and policies and of German responses. The study reflects a broad perspective, rejecting detail.

Shanafelt begins with the well-known fact that the alliance had already suffered serious stresses before Sarajevo but that its continuance was mutually beneficial. The Dual Alliance gave Austria status as a major European power and gave Germany a diplomatic buttress and bridge for the economic exploitation of the Balkans. Since Germany proved the stronger of the two, Austria resented its subordinate role. Although he takes the usual position that Austrians maintained the old dualistic system and met crises by muddling through, the author recognizes that Foreign Minister Leopold von Berchtold was not the incompetent he is so often painted. Rather, he was hesitant and apt to rely on his assistants, Counts Alexander Hoyos and János Forgách, and on the highly influential Hungarian minister-president, Stephan Tisza.

The weakest chapters of the book are the early ones, which do not offer a comprehensive picture of wartime diplomatic policies regarding the Balkan conflict. Turkey and Greece are ignored and the treatment of Bulgaria is superficial. But the author does point once again, and with some care, to the disparate views and policies regarding Romania and its desire to annex Transylvania in return for a wartime alliance. The differences between German and Austrian military prowess in the field and the diplomatic repercussions of poor Austrian performance are described.

Most worthwhile are the chapters concerned with Poland's future fate and with the conflicting views of Emperor Karl and Foreign Minister Ottokar Czernin on dealing with the Germans and on peace negotiations. The "Austrian-Polish Solution" was aimed at absorbing all of Congress Poland into the Dual Monarchy and granting the Poles equal constitutional status to thwart the ambitions of Polish patriots for independence. German Chancellor Theobald von Bethmann-Hollweg would have accepted this solution to the Polish question only if the monarchy virtually surrendered its independence by joining a Mitteleuropa controlled by Germany. Austrian refusal of the scheme resulted in German insistence on an autonomous Polish state under German domination.

The poor showing of Austrian field forces and the deteriorating situation on the homefront made pursuit of a hard-line policy toward Germany impossible after 1916. Karl and Czernin attempted a new approach: Karl sought a compromise peace for Austria, alone if necessary, whereas Czernin wanted the same peace but insisted on German concurrence. Each erroneously assumed that the Entente and Germany would scale down their war aims. Vienna's hope to mediate an early peace was dashed by German insistence on extensive compensation. Czernin could not ignore Berlin. He needed German support to avoid the monarchy's dissolution; ironically the alliance with Germany spelled the doom of Karl's empire.

Obviously the book has its faults, including insufficiently detailed citations, but it is a worthwhile addition to the existing literature. In its attempt to cover Austrian-German relations throughout the war, Shanafelt's book proves to be an intelligent, well-informed beginning but hardly an end.

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ALFRED D. LOW. *The Anschluss Movement, 1931–1938, and the Great Powers.* (East European Monographs, number 185.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. xv, 507. $40.00.

As a major topic in Austrian domestic history and interwar European diplomacy, the question of the desirability of the union of Austria and Germany and its accomplishment in 1938 has been discussed in depth in numerous books and articles. Alfred D. Low, an authority on the subject, has published two previous standard studies: *The Anschluss Movement, 1918–1919, and the Paris Peace Conference* (1974) and *The Anschluss Movement, 1918–1919: An Annotated Bibliography of German and Austrian Nationalism* (1984). In the book under review the author not only emphasizes the role that the question of Anschluss played in European diplomacy and in Austro-German relations but also discusses the importance of the issue in internal Austrian politics. He recognizes the difficult position of the independent state, which enjoyed neither the loyalty nor the support of most of its citizens; he concludes that "the lack of an Austrian patriotic ideology contributed to the ultimate demise of the First Austrian Republic" (p. xiii). Certainly, at least before Hitler's rise to power in Germany introduced other dangers, the majority of Austrians favored union not only because of sentiments of national kinship but also because they were convinced that the small Alpine state was not economically viable. Most Austrian intellectuals held similar views, including the historians Heinrich von Srbik, Oswald Redlich, Ludwig Bittner, and Hans Uebersberger. The Social Democrats did not abandon their call for Anschluss until their last party meeting in October 1933.

This book is particularly valuable for the manner in which it traces the twists and turns in the policies of the great powers most closely concerned—Britain, France, and Italy—as well as those of the Soviet Union and the United States, states that had less influence on the course of events in Central Europe and whose attitudes have received less attention in other accounts. Until 1932 the Soviet Union supported an Anschluss; thereafter, that state stood for an independent Austria and strongly opposed both the union, whose accomplishment in 1938 it never recognized, and the establishment of a Danubian federation. Although, in the author's opinion, the United States had excellent observers on the scene—ambassadors W. E. Dodd in Berlin and George Messersmith in Vienna—the U.S. policy of "aloofness and isolation" (p. 332) meant that neither the European nor the Austrian leaders expected an American influence in the area.

With the major attention given to the years from 1933 to 1938, the author discusses shifting policies of Britain, France, and Italy. Once Mussolini began to play a major role in the Danubian region, he too supported Austrian independence, particularly during the period of the authoritarian rule of Englebert Dollfuss and Kurt von Schuschnigg. The Italian resolve, however, as well as that of France and Britain, dissolved before the force of German revisionism and as a result of the Italian involvement in the war in Ethopia in 1935. By March 1938 no power was willing to risk war to prevent the union, whose accomplishment marked a major shift in the balance of power in Central Europe to the benefit of Nazi Germany.

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I. IA. ZLATKIN. *Istoriia Dzhungarskogo khanstva, 1635–1758* [History of the Dzhungar Khanate, 1635–1758]. 2d ed. Moscow: Nauka. 1983. Pp. 330. 2r. 90 k.

Despite their intrinsic interest and geopolitical importance, the border regions between Russia and China have been neglected by historians in the West. In part this is the result of inadequate language capabilities, in part of the inaccessibility of archival source materials, and in part of the tendency of Chinese and Russian historians to treat this area primarily as an arena for their own hostile relations. The peoples of the region—Muslim Turks and Mongols in China, and nomads in eastern Turkestan—have been largely ignored. Recently a number of Soviet historians have devoted their attention to the histories of these frontier peoples, with interesting and important results. I. Ia. Zlatkin's monograph on the Dzhungar (Jün Gar) Khanate first appeared in 1964, and this review concerns the second edition of that work. As the only full study of the Dzhungar Khanate in any language, Zlatkin's work is extremely important. Based in large part on Russian, Chinese, and Mongolian sources, the study examines the internal development and structure of this state (which occupied the large basin between the Tien Shan and the Altai Mountains) and the character of the chief Dzhungar elites and leaders and summarizes what little can be learned of the life and society of the population of the region during the century and a half under consideration.

Especially interesting are the characterizations of Khan Galdan in the late seventeenth century (pp. 159–82) and of the conflict between nomadic culture and the requisites of governmental authority (pp. 253–278). Likely, the most useful sections of Zlatkin's monograph for Western readers are his examinations of Russian and Chinese perceptions of the Dzhungar state, the effects their own relations had on these perceptions, and the impact first of

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Dzhungar expansion and then of that state's collapse on the configuration of the Russian-Chinese border.

Although Zlatkin recognizes the lasting importance and indeed makes substantial use of works by nineteenth- and early twentieth-century Western scholars such as E. Bretschneider, H. Howorth, M. Courant, and G. Cahen, he apparently did not have access to the growing body of scholarship produced by Uralic and Altaic specialists in Hungary, Germany, and the United States. Indeed, Zlatkin makes no use or mention of anything published in the twenty-year period since the first edition. This is a shame, for Zlatkin is clearly a scholar of considerable talent.

The author feels compelled to present this history in a somewhat anachronistic fashion as well. He argues that the Dzhungar would have been much better off under Russian than Chinese rule and that the various Russo-Chinese treaties of the period (including even the Treaty of Nerchinsk) ought to have had conclusions more favorable to the Russians. At the time, however, the Dzhungar believed that neither was preferable, that both China and Russia were to be avoided if at all possible. It is not very helpful for historical understanding to transport twentieth-century problems and attitudes into the seventeenth or eighteenth centuries.

Nevertheless, since Zlatkin's book stands alone, it is required reading for historians who wish to understand Russian-Chinese relations and to appreciate the importance of the frontier peoples between the two states in the premodern era.

ALAN FISHER
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V. I. TIMOFEENKO. *Goroda severnogo Prichernomor'ia vo vtoroi polovine XVIII veka* [Towns of the Northern Black Sea Region in the Second Half of the Eighteenth Century]. Kiev: Naukova Dumka. 1984. Pp. 217. 2 r. 90 k.

This book surveys the origins and early evolution of the various towns of the southern Ukraine—or New Russia as it was frequently termed in the eighteenth and nineteenth centuries—largely from the perspective of town planning, particularly as concerns site selection and land use. V. I. Timofeenko contends that his principal concern is to delineate the particularities of these historical cities that should be preserved in the current process of reconstruction and urban development. He draws on diverse materials, most of them published, and also cites many unpublished plans, few of which are reproduced. In structure the study is clearly (albeit conventionally) organized. Two introductory chapters sketch the Russian conquest of the region and the role of fortresses as precursors of urban development. The towns are classified according to a fourfold typology: administrative centers, trading towns, military towns, and artisan-trading settlements. The author pays more attention to the larger towns, such as Odessa, Sevastopol, Kherson, and Simferopol, but discusses many others too. In three chapters with self-contained conclusions he treats the towns by region: those east of the Dnieper River, those west, and those on the Crimean Peninsula. A short discussion of "the principles of the foundation of cities" and a brief conclusion complete the work.

Supplementing the text are scores of illustrations—plans, maps, prints, diagrams of reconstructed buildings, photographs. These might have enlivened Timofeenko's work had they not been of such mediocre quality: they lack scales, adequate identification, or even much discussion in the body of the work and are dark and fuzzy. The single regional map is small, crowded, and difficult to read; it compares badly with those in recent works by E. I. Druzhinina and Roger Bartlett.

In conception the book seems quite conventional, even provincial. Timofeenko refers to numerous architects and military engineers with obviously foreign names, but he rarely identifies them and often implies that they were Russified or that they simply followed their Russian superiors' orders in selecting sites, plans, and styles. He also mentions classicism several times but does not explore its international influence or its special attractions to eighteenth-century Russians. Foreign scholarship is totally ignored. Ukrainian folk styles and customs are cited several times, but the influence of Russian or European town planning is mostly overlooked. The entire process of town planning and construction looks remarkably bland, even though the author occasionally refers to unfavorable conditions—lack of potable water, timber, and sewerage, for example, as well as marshy terrain, epidemics, and famine. The multi-ethnic composition of the population of these towns is noted more than once, yet the author denies its determining influence on urban styles and plays down any ethnic rivalries or conflicts. Scant social history, anecdote, or biographical excursions break the author's monotone. Although based on a wide selection of material, the book lacks an index or bibliography. Perhaps the work has some value for historians of architecture and town planning. Its value for general historians, much less for any broader reading public, appears extremely limited.

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J. H. SEDDON. *The Petrashevtsy: A Study of the Russian Revolutionaries of 1848*. Dover, N.H.: Manchester University Press. 1985. Pp. viii, 312. $30.00.

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J. H. Seddon's book is explicitly intended as a piece of revisionism. It seeks not merely to clarify the ideas and activities of the Petrashevtsy but also to rescue them from obscurity and to substantiate the claim put forth at the beginning of the book that "the Petrashevtsy were the first large group of socialists and revolutionaries in Russia" (p. 15). Such a claim, if true, would in turn seem to necessitate a reappraisal of the Russian revolutionary movement as a whole and the inclusion of the Petrashevtsy with Belinskii, Herzen, and the Decembrists as the principal forerunners of the radicalism of the late nineteenth century.

Seddon is largely successful in her effort. As she properly points out, the Petrashevtsy were more than the handful of persons who, on Friday evenings in the late 1840s, prior to their arrest in 1849, visited the apartment of Mikhail Petrashevskii for the purpose of discussing political and philosophical issues. Rather, through the network of overlapping circles that they established, the Petrashevtsy included several hundred activists intent on disseminating propaganda among the masses in preparation for a general insurrection—even though a small minority, led by Nikolai Speshnev, stressed instead its preference for an immediate coup d'état. Although their efforts did not amount to much, as Seddon acknowledges, the Petrashevtsy left behind a corpus of writings and various models of revolutionary organization that subsequent generations of revolutionaries, largely through the mediation of Chernyshevskii, could draw on in accordance with their own experiences and needs. Indeed, in their belief that the peasant commune (*obshchina*) could be the basis for a socialist society, the Petrashevtsy clearly prefigured the argument of the Populists and others that material life in Russia could be improved without an intervening stage of capitalist development.

Seddon's book is instructive in other ways. Her research, which is exemplary, amply substantiates the intriguing parallels she draws between the ideas of the Petrashevtsy and those of Marx. Equally competent is her analysis of the French socialists—Fourier, Proudhon, Saint-Simon, Blanc—from whom the Petrashevtsy borrowed extensively, if also quite selectively, in formulating a program that they thought was relevant to the harsh political realities of Russia. Finally, Seddon gives the reader a sense of what the Petrashevtsy were like as human beings without indulging in speculation about the psychological roots of their personalities—something for which sufficient evidence is apparently not available. The book is clearly written and refreshingly free of jargon.

Nevertheless, one must take to task the editors of Manchester University Press for publishing a book without having proofread it carefully. The text has far too many typographical errors. In addition, the author commits several small, though glaring, factual errors, misdating Pugachev's revolt (p. 205), the judicial reforms of Alexander II (p. 235), and the publication of Dostoevskii's *Crime and Punishment* (p. 242); in 1878 Vera Zasulich shot Fedor Trepov, not Dmitrii Trepov (p. 242).

These failings aside, Seddon has rendered a portrait of the Petrashevtsy that enables the reader of her book to situate them precisely in the growth and development of the Russian revolutionary movement. For this reason, her book is a welcome addition to the historical literature on this subject.

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JEFFREY BROOKS. *When Russia Learned to Read: Literacy and Popular Literature, 1861–1917*. Princeton: Princeton University Press. 1985. Pp. xxii, 450. $37.50.

Does Russian popular literature provide an appropriate medium through which to discern the appearance of a new literate culture in the last half-century of tsarist rule? In this thoughtful and creative study Jeffrey Brooks argues that it does. In the first section he traces the emergence of the new "boulevard literature"—so-called by its upper-class critics—and the rise of a mass reading public. The second part explores four important themes in this popular fiction that in his opinion reveal changes in the "inner world" of the literate lower classes. This thematic analysis constitutes the book's most original contribution to Russian cultural history and is likely as well to arouse the greatest debate regarding both his methods and his conclusions. The last section examines the opposition to popular commercial literature, which on the one hand defied tsarist censorship policies and on the other challenged the "culturism" of intellectuals seeking to mold the mass mind in their own image.

The author has an easier time surveying the array of popular literature available in the late nineteenth and early twentieth centuries than describing the "typical" reader. His tables outline clearly the increase in the number of titles and the evolution of various genres of popular reading. The most notable trend is the sharp decline of religious texts and the rise of "pulp" fiction. Lacking any concrete information on the consumers of this literature, Brooks has to infer readership from the mode of distribution (chapbooks for the peasantry, newspaper serials for the urban readers, and so on) and the content of the books. He does not consider class differences crucial to a profile of the ordinary readers, who in his view are distinguished primarily by "a more cosmopolitan orientation toward the world around them" (p. 165).

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This new reading public exercised what he considers a form of consumer sovereignty. Publishers sought profits by pandering to the literary taste of their consumers. They employed—at miserly pay—writers attuned to and able to satisfy the cultural needs of this public. Brooks infers that a close cultural rapport existed between the readers and writers of popular literature. Having made this key argument, he then proceeds to explore the symbolic meaning of four "imaginative formulas" prevalent in these stories.

His selection is based on a quantitative analysis of the contents of many hundreds of publications, from which he selects a more restricted number for careful analysis. Implicit in this reading is the approach developed in recent years by reader-oriented literary critics. Although Brooks does not elaborate on his procedures, he does accept the basic premise of this literary school that the text itself provides insight into the reader's values and expectations.

The four themes he identifies include the conflict between freedom and order in bandit stories, new images of Russian national identity, the struggle of science against superstition, and individual aspirations for success. As he recognizes, the subject of the doomed bandit echoed a traditional theme in Russian folk tales and evolved only slightly in the early twentieth century toward justification of the rebellious individual. By contrast, a new theme of individual success developed and expanded in scope as the decades passed. Frequently associated with patronage and learning, it emphasized social mobility and extolled materialistic standards of worth and individual achievement, images of success that Brooks believes were characterisic of a growing segment of the Russian population.

The vulgar materialism and sensationalism of popular literature at the turn of the century was not at all to the liking of many intellectuals. They shared a disdain for this diverse mass culture, to which they opposed a dream of "cultural wholeness" to be promoted by a select, "sponsored" literature of their choosing. Their opposition to popular literary taste triumphed after the revolution of 1917 only because the Bolsheviks imposed their own "culturist" standards and within a year eliminated the prerevolutionary popular literature.

This book opens new perspectives on Russian literature and culture. Some scholars will object to its claim to have identified new cultural values through the study of popular fiction. The limits to Brooks's method of literary analysis are those encountered in any study of the attitudes of Russian lower classes in the late tsarist period, namely, that the verifiable sources—in this case the texts—are the product of a few individuals. Although the authors were themselves of humble origin and sought subjects that appealed to a large public, their handling of the themes revealed their own cultural perceptions and horizons. The readers' response to the new fiction is unknown. Yet the effort made here to link popular literature with its social context appears a fruitful area of investigation in Russian cultural history, one already explored with great profit by historians of Western cultures. This study provides both a challenging interpretation of Russian popular literature and a model for future research in this field. It represents on both counts an important contribution to Western historical works on the history of late imperial Russia.

DANIEL R. BROWER
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HEINRICH SCHERER. *Der Aufbruch aus der Mangelgesellschaft: Die Industrialisierung Russlands unter dem Zarismus (1860 bis 1914)*. (Focus Kritische Universität.) Giessen: Focus. 1985. Pp. 350.

This analysis of Russian industrial development has several strengths. It provides dozens of statistical tables, drawn from various secondary works, showing trends in production, taxation, the public debt, the size of the labor force, and other indexes of economic change. In assessing Russian industrialization, Heinrich Scherer usefully notes that, although coal production increased 1100 percent between 1880 and 1913, per capita coal output in 1913 (209 kilograms) lagged far behind that of Germany (2822) and the United States (5358); some sectors of Russian industry actually regressed in comparison with Europe and North America. The author also stresses the disparity between stagnant agriculture and highly capitalized heavy industry.

Scherer does not adopt the superficial view that the tsarist regime consistently encouraged capitalist modes of production. In a critique of the orthodox Soviet interpretation of "monopoly capitalism," he follows Heiko Haumann in arguing that the proliferation of cartels did not signify the maturation of Russian capitalism. Instead, the tsarist bureaucracy consistently hampered the free development of capitalist institutions through its heavy taxation for state expenditures (especially armaments), its excessively high import tariffs, and its resistance to capitalist forms in agriculture. In this regard *Ambivalenz* and *Diskrepenz* are two of his favorite words.

These strengths fail to compensate, however, for the author's overly schematic presentation. The ambivalence that characterized tsarist economic policy cannot be proved simply by citing the number of rubles devoted to various items in the state budget. Although Scherer consulted an impressive array of secondary works, mostly in German and English (he does not read Russian), his research is incomplete,

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especially in the biographical literature. The brief footnote on the so-called Russian bourgeoisie, an object of much scholarly attention in recent years, includes no works published after 1973 (p. 319 n. 77). Arguing that excessive and irrational military expenditures hindered economic development, the author cites two works by Seymour Melman published in the mid-1970s. It implies no criticism of Melman's work to suggest that Scherer might have strengthened his argument by quoting instead the leading pacifist of late imperial Russia, the railroad entrepreneur and banker Jan Bloch (Ivan S. Blokh). Historians with a statistical inclination will question the author's uncritical use of P. V. Ohl's dubious calculations regarding foreign investment. His assumption of fatal weaknesses in the tsarist economic system before 1914 leads Scherer to ignore the extraordinary strains imposed by the Great War.

These methodological problems apparently stem from the author's preoccupation with what he regards as the major lesson of the Russian experience: the alleged impossibility of undertaking industrialization within the international capitalist system. In his opinion, unsupported by any evidence, "the Soviet experiment" demonstrates that "a temporary separation from the imperialist economic sphere ensures an improvement in the national productive system and the provision of basic necessities to the population" (p. 82). To Scherer, "the significance of the October revolution for the developing countries" consists in the Bolsheviks' "withdrawal from the world market in order to protect national interests and to build up domestic industry" (p. 327 n. 153). In this spirit he adheres to quaint terminology ("primary accumulation" and post-1861 "feudalism") and dismisses the work of Alexander Gerschenkron as an "offshoot" of the anticommunist American theory of modernization. By what logic do the economic irrationalities of the tsarist autocracy justify those of the Bolshevik autocracy?

This question is intended neither to disparage the intelligent use of Marxist categories in economic history nor to imply that historians should ignore the political implications of their research. All the same, the structure and tone of this book might tempt librarians to shelve it in the section reserved for German anticapitalism rather than Russian economic history.

THOMAS C. OWEN
Louisiana State University

PIERO MELOGRANI. *Il mito della rivoluzione mondiale: Lenin tra ideologia e ragion di Stato 1917–1920.* Rome: Laterza, 1985. Pp. xvi, 236. L. 13,000.

Piero Melograni, a non-Marxist political scientist, is the author of several books on World War I and on the Fascist era in Italy. In the present book he broadens his focus to examine Lenin's ideological conception and political use of the "myth" of world revolution. Most of the research was done at the Woodrow Wilson Center in Washington in 1980. Melograni argues that Lenin was, above all, a realist and pragmatist, unlike Nikolai Bukharin of the Comintern and a number of other Communists. According to the author, Lenin did not believe that a Bolshevik revolution was likely to occur in Europe, and he was indeed hostile to giving it encouragement. An abortive revolution, such as that of Béla Kun in Hungary in 1919, would be harmful to Lenin's chief foreign policy goal, which was to gain security and stability for Soviet Russia and a general political settlement with the Western powers. Lenin wanted to get the capitalist world to extend diplomatic recognition to Soviet Russia, to trade with it, and to accept it as a full member of the international community. In short, he favored "coexistence." Moreover, he sensed that, if Communists did come to power in the Western countries, they would likely not see eye to eye with the Kremlin. He points out that this would almost certainly have been the case with Rosa Luxemburg.

Lenin's establishment of the Comintern in March 1919 was designed not so much to promote imminent world revolution in Europe as to split the Western Marxist parties and bring the new Communist parties under the control of the Comintern, where they would be rigidly disciplined and made to support Soviet Russia's foreign policy without question. Melograni's argument that Lenin opposed a Marxist revolution in Italy in 1919 has been the subject of considerable debate. Paolo Spriano, a Communist historian of the Italian Communist party, has expressed a number of reservations. Nevertheless, Melograni has found at least partial support for his views from such historians as Gastone Manacorda, Paolo Alatri, and Leo Valiani. Melograni concedes that Lenin did see some advantages in encouraging revolutions in Persia, Afghanistan, and Iraq as a way of gaining leverage over the British empire in his long-term goal of securing a general peace settlement with the capitalist world.

In his tightly argued book Melograni repeatedly makes it clear that he is focusing his study on Lenin. He does not deny the possibility that certain other Bolshevik leaders did take seriously the possibility of an imminent world revolution.

CHARLES F. DELZELL
Vanderbilt University

ALEXANDER YANOV. *The Drama of the Soviet 1960s: A Lost Reform.* (Research Series, number 56.) Berkeley: Institute of International Studies, University of California, 1984. Pp. xix, 141. $8.50.

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This is a stimulating and perceptive study of the economic and political reforms of the Khrushchev era. Alexander Yanov gives Khrushchev high marks as a reformer: "He declared war on the anti-reformist elites and fought them with an artfulness much greater than any other Russian reformist leader in the twentieth century: certainly greater than Stolypin in the 1900s, or Lenin after 1921, or Bukharin in the 1920s, not to mention Malenkov and Kosygin" (p. 122).

Although it also deals with reform efforts in general, economic administration, the military, and party organization, the book concentrates on Khrushchev's agricultural reform, chiefly the link system. The descriptions of agricultural conditions and of the reform efforts applied to them benefit from the author's extensive personal observations of Soviet agriculture as a journalist.

Yanov writes of the efforts in the 1960s to break out of the constraints of the kolkhoz system, with its swollen labor force and low crop yields. He describes the heroes of the reform effort—Vladimir Pervitskii, Ivan Khudenko, and others who worked doggedly first to demonstrate the superiority of the link system (in which small groups of workers assumed a specific production or work contract, paid themselves from their earnings, and therefore were motivated to maximize both their production and their income), first within the established kolkhoz and then outside it. Demonstration of the efficiency of the links was the easy task; the trouble came when vested interests—the protected labor force of the established farms and the officials of the party and state hierarchy who ran the system—perceived the threat to themselves. Hence the hasty abandonment of the reform effort under Brezhnev: "The Stalinist leadership of 1929 responded to the challenge of peasant differentiation by legislating NEP [New Economic Policy] and instituting forced collectivization. The post-Khrushchev leadership replied to the challenge by suppressing the Link Reform and restoring the status quo" (p. 68).

Although Yanov provides greatest insight into Khrushchev's agricultural reforms, he is also concerned with a broader reform effort that he describes as "Khrushchev's across-the-board challenge to the largest shareholders of USSR, Inc." That challenge, which ended with Khrushchev's removal in 1964, had other facets: a revival of de-Stalinization, a challenge to "the 'priestly' hierarchy in the field of culture . . . the reduction of military expenditure and the declaration of war against the 'steel-eaters'" (p. 81).

Khrushchev had apparently decided that his policies, including agricultural reform, would move forward only if he could change party personnel (hence the revision of party rules limiting the tenure of party professionals in their posts in 1961) and reduce the powers of rural party secretaries over the professional managers (abolition of the rural party district committees in 1962). But, by alerting party professionals to the challenge his policies posed to their powers and their tenure, such measures were Khrushchev's undoing. Khrushchev activated the antireform segment of the party but "failed to consolidate and institutionalize" what Yanov describes as the "reformist establishment" (p. 109).

Khrushchev's "painful and dangerous attempt to break through to a genuine reform" in agriculture is Yanov's main theme, but he sees this effort as part of a continuous pattern of Russian history in which reform elements vie with one another in the establishment, a manifestation of a "basic dualism of Russian political culture" (p. 108). On balance the book is more effective for the insights it provides into the details of Khrushchev's agricultural policy than for its excursion into broader theory about Russian history and political development or its critique of American Sovietology, although his criticism has some merit. Yanov poses important questions about the problems of reform of the Soviet political system, the role of the key institutions in blocking change, and the divisions within the establishment on reform issues. The book provides impressive evidence on the way in which the insights and experience of a Soviet émigré—especially one conversant with Western Soviet studies—can contribute to the study of Soviet history and institutions.

HERBERT J. ELLISON
University of Washington

NEAR EAST

MARTIN KRAMER. *Islam Assembled: The Advent of the Muslim Congresses.* (Dayan Center for Middle Eastern and African Studies.) New York: Columbia University Press. 1986. Pp. xi, 250. $30.00.

This is a wide-ranging study of international "congresses" held during the interwar period to assert Muslim solidarity vis-à-vis the triumphant imperial West. The idea of such meetings to foster cooperation among the community of Muslim believers (*umma*) originated about 1880. Sultan Abdulhamid II's claim to be caliph of all Muslims sprang from a similar impulse. But the sultan feared that an international Muslim congress might threaten his caliphal claim; therefore, no congress convened until after the Ottoman collapse in 1918.

According to Martin Kramer, the idea of the congresses was a Western import that took root among the Muslim associates of the English aristocrat Wilfred Blunt. Jamal al-Din al-Afghani's heirs, Muhammad Abduh and Rashid Rida, popularized

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the idea. British administrators also toyed with the possibility of using such a convention for their own ends. Following an argument developed by Elie Kedourie and others for al-Afghani's circle, Kramer notes that the congress theme "appeared in every instance in intimate association with reputed freethinkers, rationalists, skeptics, and non-Muslim visionaries" (p. 25).

Muslim congresses eventually assembled at Mecca, Cairo, Jerusalem, Geneva, Ankara, Moscow, and even Tokyo. Rashid Rida, Shakib Arslan, Hajj Amin al-Husayni (the Mufti of Jerusalem), and the Khilafat leaders from India were most active in the movement.

Kings Husayn ibn Ali, Abd al-Aziz Ibn Saud, and Fuad sponsored congresses to assert their own claims to international Muslim leadership after the Turks abolished the caliphate in 1924. Private individuals with doubtful mandates from their countrymen provided uneven representation of regions outside the host countries. Political disputes damaged the congresses, none of which reconvened or established effective permanent executive branches.

Kramer has done an admirable job of tracking down published and archival records; the former palace archives of Egypt are a welcome addition to the usual list. The interpretation is generally sound and dispassionate. The desperate circumstances that drove the Mufti and Shakib Arslan to their disastrous collaboration with Hitler and Mussolini might have been brought out more clearly.

An added paragraph or page here and there might have placed the study in a wider context. The international congresses of orientalists, which convened from the 1870s on, could be seen as a counterpoint to (and possibly an impetus for) the international Muslim congresses. So could the overtly political Congress of Berlin (1878), whose participants ignored Ottoman points of view. Middle Eastern constitutions and national assemblies also appeared at about this time. One should not overlook the reinterpretation of classical Muslim ideas, such as *shura* (consultation), *ijma'* (consensus), and *majlis* (council, parliament), that helped acclimate congresses, constitutions, and parliaments to the Middle East. What, for example, were the earlier associations of the word *mu'tamar* (congress)?

Kramer sensibly concentrates on the interwar pan-Islamic congresses, but more than a passing mention of specifically Arab congresses would have been interesting. A page or two on Muslim congresses since 1945—Kramer notes that these have been governmental rather than individual initiatives—would also have been enlightening.

In conclusion, Kramer has written an original and stimulating book. It stands as a significant contribution to the field and, like most good studies, points the way for further research.

DONALD MALCOLM REID
Georgia State University

JOHN D. MCINTYRE, JR. *The Boycott of the Milner Mission: A Study in Egyptian Nationalism.* (American University Studies, ninth series, History, number 10.) New York: Peter Lang. 1985. Pp. viii, 214. $28.00.

John D. McIntyre Jr.'s book is an account of the boycott of the Milner Mission by the Egyptians in 1919–20. It begins with two chapters on historical background, then goes on to discuss the attitudes of various groups within Egyptian society—such as Moslem and Coptic leaders, women, students, and the Wafd central committee—toward the Mission. McIntyre's study is based on both British and Egyptian primary sources as well as secondary works. The basic contribution of the book is its study of the role of the Wafd central committee and its most controversial member, Abd al-Rahman Fahmi.

The central committee, which was originally founded to raise funds and information in Egypt, while the founding members of the Wafd were in Paris, soon acquired a life of its own. Under the able leadership of Fahmi, who was not a founding member of the Wafd, the central committee became a center of power within the Wafd. Because of his active agitation against the British, Fahmi was accused of heading a secret terrorist society. This is an interesting book, because it relates previously unknown details on the events of the period, which highlight and clarify some of the incidents.

I would disagree only with McIntyre's conclusion that the Coptic community joined the nationalist movement not out of conviction but out of fear of the Moslem majority. As evidence for that claim, he adduces the accounts of some Protestant American missionaries. It was well known that the missionaries and the Coptic community did not see eye to eye. The missionaries were there to convert the Copts so that inevitably friction arose between the two groups. Those Copts who accepted conversion were looked on with disfavor by the rest of the Coptic community. Thus, it is difficult to accept accounts by the missionaries as the criterion for deciding the reasons for the Copts joining the nationalist revolt (p. 116). People join nationalist movements for a variety of reasons and, although some Copts no doubt joined out of fear, others joined because of nationalist sentiment as much as did the Moslems.

The book could have been better edited, for instance, to avoid two spellings for "consensus," one found on the back cover of the book. Egyptian names are spelled erratically ("Sinut" and "Sinot" pp. 37 and 112) as are Arabic plurals ("kuttabs" and

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"katatib" pp. 103–04). The date on page 17 should be 1918 not 1919.

One passage is erroneously translated (p. 98). The reference in the Arabic original is to shoes and weapons, not shoes and socks. On page 114 Armenian massacres are brought in out of the blue with no explanation of why they are there. Lastly, the incident Russell recounts in his memoirs, certainly did not refer to Huda Shaarawi but to a younger person, probably Hawa Idris (p. 183).

AFAF MARSOT
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AFRICA

CHARLES L. REDMAN. *Isar es-Seghir: An Archaeological View of Medieval Life*. Orlando, Fla.: Academic. 1986. Pp. xi, 259.

Isar es-Seghir was a small, walled port on the northern coast of Morocco, almost equidistant between Tangier and Ceuta. The town contained a significant Muslim population from the late twelfth century A.D. until 1458, when it was captured by the Portuguese, who occupied the site until its abandonment in 1550. Charles L. Redman has directed the excavation of the site since 1974, and this book is a popular account of the excavation and his findings. Detailed study of the various aspects of the undertaking are apparently to appear as *Isar es-Seghir: The Islamic Period* and *Isar es-Seghir: The Portuguese Period* in *Villes et sites: Archeologique Morocaine*. The present work is "a tale of two cities," the successive but distinct Islamic and Portuguese phases of the town's existence. The historical background, the architectural features, and the material remains (primarily ceramics) are given for each phase. The most interesting part of the book is the concluding section where the author compares the two periods of settlement.

The major objective of the author, however, is to tell "two tales of a city," that is, to write a site report and an introduction to the methods of "anthropological archaeology." It is impossible to judge whether this work is an adequate summary of the excavation, but most historians are grateful for archaeological evidence and for its prompt publication, especially of Islamic sites. Unfortunately, the claims for anthropological archaeology, which is never clearly defined, are intrusive. Although anthropology can surely make an important contribution to modern archaeology, the advantages of this new discipline are unclear from the present study.

The traditional descriptions of the historical context of the site are the weakest parts of the book. Redman does not discuss what he means by the term "ancient city," and it is arguable that Isar es-Seghir was neither. He does not define the terms "public" and "private," although he devotes a good deal of attention to these notions in the discussion of the spatial arrangements in the town. He also makes a number of factual errors. In the second chapter, which gives the historical background to the Islamic period, the following points may be noted. The conquering Arabs could not have brought Islamic law to the West in the eighth century because the law developed only much later (p. 22). The 'Abbâsid capital was in Baghdad, not Damascus (p. 23). Ibn Tashufin is normally transliterated as Ibn Tâshfîn (p. 26), Abu Yusuf Yacqub as Ya'qûb (p. 27), Las Navas de Tolo as Tolosa (p. 29), and the name of the site in classical Arabic would be "al-Qaṣr aṣ-ṣaghîr" (p. 6). The discussion of European history in the fifteenth and sixteenth centuries is embarrassing; for example, would most historians say that "the territorial expansion of Europe was seen as a precondition for the solution to the crisis of feudalism, and without it Europe might have collapsed into relatively constant anarchy and world history would have taken a sharply different course" (p. 32)? Is it possible, as Redman suggests (p. 42), that the population of Isar es-Seghir reached a maximum from approximately 1350 to 1458, when the Black Death and plague recurrences were afflicting the rest of the Old World? Furthermore, there is no evidence of craft guilds in medieval Islamic cities (p. 48), and no support for the view that "the pervasiveness of Islamic law from Spain and Morocco in the west to Arabia and Iran in the east created an enormous common market of mutually intelligible language, uniform law, and standard weights and measures" (p. 106). In general, most nonspecialists, for whom this book is intended, would probably have liked to know more about excavations of other Islamic sites, particularly comparable ones in North Africa and Andalusia.

Despite these shortcomings, Redman gives us a sharply focused view of the architectural and material culture of a small Moroccan town in the later Middle Ages and a careful assessment of its evolution.

MICHAEL W. DOLS
Corpus Christi College, Oxford University

DENNIS D. CORDELL. *Dar al-Kuti and the Last Years of the Trans-Saharan Slave Trade*. Madison: University of Wisconsin Press. 1985. Pp. xvii, 283. $35.00.

This is the story of a small nineteenth-century kingdom, isolated in the dry grasslands between the Niger and the Nile rivers in what is today the

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Central Africa Republic. Study of this "periphery of a periphery" presents an opportunity to examine carefully the terrible effects of the arrival of intensive slaving and the use of firearms in a kin-based, agricultural society. Here we see close up the violent frontier of expansion of the militarily superior Middle East and Europe.

Dennis D. Cordell leads the reader through a survey of the Ubangi-Shari region in the nineteenth century to focus on Dar al-Kuti and its capital, Ndele. First, he describes the traditional life of the Banda, Nduka, and other peoples in the region and discusses the spread of Islam in the early part of the century as part of a peaceful growth of regional trade in cloth, pottery, iron goods, and other local products. Cordell then shows that in the 1870s three new forces began to influence the region: the growing power of Egypt, the drying up of slave sources in other parts of Africa, and the European demand for ivory. As a result, the whole region west and south of the Upper Nile was caught in an ever-widening circle of violence. Those who could secure a supply of guns and ammunition could compete for wealth and power previously unknown in the region.

About half of the 161 pages of text are devoted to life in Dar al-Kuti under Muhammad al-Sanusi. Early in his career al-Sanusi ruled over Dar al-Kuti as a deputy of Rabih B. Fadlallah, the first slaving warlord in the northern Ubangi-Shari. In 1891 al-Sanusi massacred a French expedition, took their large supply of arms, and broke away from Rabih. He raided to the south and west in search of slaves and recruits for his regiments. Ndele, a rocky fortress town, became his capital in 1896. As successful as it was, however, the kingdom of Dar al-Kuti only lasted into the early twentieth century. When the French began to restrict the slave trade, al-Sanusi was forced to oppose them because his power depended on exchanging slaves for firearms. In 1911 he was assassinated by the French representative at Ndele, and his followers were scattered. Cordell shows that al-Sanusi organized his Muslim and non-Muslim subjects into an extensive raiding, trading, and plantation economy. This section is full of detail on topics such as currencies, the methods of raiding, the treatment of subjects, the volume and prices of goods exchanged, the layout of the capital, and the role of Islam.

This concise descriptive history is written in a pleasing style and is based on careful research. Although Cordell, like most Africanists, worked with limited data, he searched intensively for unpublished sources: he visited eleven archives in Europe and Africa, collected interviews in and around Ndele, and surveyed the Ndele ruins. He provides carefully prepared maps, plans, genealogies, tables, and charts.

In a brief conclusion Cordell refers to his major themes that could be of interest to others. He mentions, for example, the impact of European technology, the effects of the slave trade on African societies, the importance of local political and economic organization to the long-distance trade in slaves, and the spread of Islam despite conditions of violence. A more complete discussion of how the case of Dar al-Kuti comments on the work of others would have been useful, but this is a minor flaw compared to the value of Cordell's description of this previously neglected area of Africa.

CURTIS A. KEIM
Moravian College

NORMAN ROBERT BENNETT. *Arab Versus European: Diplomacy and War in Nineteenth-Century East Central Africa*. New York: Africana. 1986. Pp. viii, 325.

The topic of confrontation between Arabs and Europeans in Africa has lost little of the interest that made it so compelling in the nineteenth century, and Norman Robert Bennett is particularly well qualified to delineate its historical contours in East Central Africa. Bennett and his graduate students at Boston University have been working in this field for the last three decades, and this volume brings together an impressive compilation of previous research and new original documentation from various European sources.

Bennett's major goal is to revise the picture of this encounter created by its European protagonists and preserved by an earlier generation of Africanist historians such as Roland Oliver. According to the received account, Europeans in East Central Africa were all David Livingstones, engaged in a moral crusade against a destructive, cruel, and decadent regime of half-breed slave traders. Bennett tries to show that, on the contrary, the Arabs of East Africa (regardless of racial lineage) were often dignified, cultured individuals who had constructed a loose but "flourishing empire benefiting, through commerce, most of its adherents" (p. 123). European explorers, missionaries, and merchants depended, Bennett argues, on Arab enterprise, generosity, and tolerance for carrying out their diverse endeavors. Ultimately, European imperialist aggression, not the intrinsic flaws of Arab institutions, brought a violent end to the Zanzibar sultanate on the East African coast and the various Muslim commercial and political settlements established as far inland as eastern Zaire.

Taken in its own terms, Bennett's argument certainly represents an improvement over the work he sets out to criticize. That it will "remain for many years the standard account," as the publisher's blurb promises us, is, however, unlikely. For one thing, it

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is mostly rather dull reading. As implied in his subtitle, Bennett takes a largely political approach to the subject but is hampered by the complex, fragmented nature of the encounters he must describe, as well as by an absence of narrative flair in his own writing. The work comes most to life in its individual vignettes rather than in its broader scope, but the vignettes in several of Bennett's earlier biographical studies are more satisfactory than those in the present magnum opus.

The central issues that Bennett confronts also require a broader, more conceptually imaginative approach than the one he employs. One can congratulate Bennett for his good sense in not succumbing to fashionable postures of making his Arab figures into African nationalist heroes, explaining all local developments through Marxist dependency theory, or belaboring the ethnocentricity of existing European sources by references to Edward Said's *Orientalism* (1978). Nonetheless, some use of the rich existing body of economic and anthropological literature is needed here to illuminate more seriously the nature of the political order created by the Arabs, the relation between their commercial system (whether concentrated on slaves, ivory, or agricultural goods) and the social organization of production in East Central Africa, and the content and role of the Islamic culture that Bennett claims defined their presence in the region. As with many revisionist historians Bennett remains, more than he seems aware, the captive of those he sets out to criticize. We emerge from his book convinced that the European "crusaders" misrepresented the regime they were destroying in nineteenth-century East Central Africa, but what that regime actually was (or, for that matter, why it was destroyed) still remains unclear.

RALPH A. AUSTEN
University of Chicago

CRAWFORD YOUNG and THOMAS TURNER. *The Rise and Decline of the Zairian State*. Madison: University of Wisconsin Press. 1985. Pp. xix, 500.

Crawford Young and Thomas Turner masterly analyze the establishment and the working of the Zairian state. The authors show in great detail that, where a set of national political institutions is supposed to exist, a series of exploitative mechanisms serving particular, very often private, ends also arises. In a way that enhances the book's originality the authors show that individual interests have common roots and should be analyzed in the wider perspective of the world system. Mobutu Sese Seko's claim to be the key element of the articulation of the Zairian state to the world system is, indeed, the critical element in the country's internal and international legitimation. The book demonstrates that Mobutu's claim to be the guardian of law and order in the country of "seven scourges" plays a central role in state and party ideology. The authors rightly insist on the deep-rooted colonial origins of this system. Present-day Zaire is the most neocolonial of African states, just as the Belgian Congo was the most perfect of colonies.

Contrary to many theoretical speculations during the 1970s, the book demonstrates that the Zairian state is not the result of a political plot perpetuated either by Mobutu or by some national or international group. The authors masterfully present the political history of the painful search for a viable compromise between international recognition and the mercantilist ambitions of a handful of individuals who have seized control of the neocolonial economic exploitative system whose authentic nationalist veneer wears thin with verbal inflation.

The first chapter of the book is a brilliant synthesis of theoretical and historical analyses of the colonial and postcolonial state. The remaining twelve chapters form three parts. In chapters 2 to 5 the authors discuss social and political forces, focusing mainly on concepts of civil society and its class and ethnic dimensions. The increasing disparity between the image of the social forces that the state cultivates and the reality of civil society carry the elements of a terrifying social explosion. The second section (chaps. 6 to 9) presents the anatomy of the main elements ("presidential brotherhood," the party as ideological tool, local administration, and security forces) of Mobutu's state. The third part (chaps. 10 to 13) analyzes the local and the international mechanisms for the extraction of surplus as well as the economic bases of the Zairian state and of its president's personal wealth.

The book takes up where Young's invaluable *Politics in the Congo* (1965) left off. The present book's empirical basis has already been stressed. It is important to add that the book adopts a diversified comparative perspective and demonstrates a rich knowledge of the latest theoretical developments. This brilliant treatise on the relations between patrimonial state and civil society in the world system will become an indispensable reference work for any student of the contemporary state. It is one of the best works, if not the best work, on the contemporary state in Africa, for it is one of the rare books that avoids, on the one hand, a theoretical analysis divorced from reality and, on the other, an empirical analysis devoid of conceptualization.

B. JEWSIEWICKI
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LEONARD THOMPSON. *The Political Mythology of* Apartheid. New Haven: Yale University Press. 1985. Pp. xi, 293. $22.50.

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The history of Afrikaners has been well documented. Much has been written about their emergence as a people, their intransigence, and their confrontations with the British and with different African ethnic groups. In his analysis of two central Afrikaner political myths Leonard Thompson attempts to grapple with the ideas and beliefs that animated Afrikaners.

The National party won the general election in 1948 on the promise to enact discriminatory legislation and because of the support of most Afrikaners. The party remains in power today. Afrikaner solidarity was nurtured by the ministers of the Dutch Reformed churches; the emergence of Afrikaans; a sense of victimization, especially after the Boer War in 1902; and the *Broederbond*, a secret organization founded in 1918 that aimed at ensuring the survival of the Afrikaner nation by vigilantly protecting its values and by attempting to place *ware*, or true Afrikaners, in leadership positions. Thompson now adds political mythology, or the exploitation of history for political ends, to these other ingredients. Political myths, which are ubiquitous in the modern world, "wax and wane in intensity . . . myths that are peripheral to the ideology may disappear when dominant classes perceive that they ceased to serve their interests, whereas myths that are integral to the ideology are far more tenacious" (p. 23).

The first political myth that played a central role in Afrikaner thinking concerns an amateurish and somewhat gruesome execution in 1815 by the British at Slagtersnek (Butcher's Neck) of a small group of Afrikaner dissidents who tried to set up a separate state. Thompson demonstrates that this historically erroneous myth has ceased to serve Afrikaner interests now that Afrikaners have gained political control and Britain is no longer a political obstacle for them.

The more enduring myth is the second, which, according to Thompson, has all of the hallmarks of a classic political myth: "partial concordance with historical reality, delayed codification, followed by rapid development and fervent deployment of political purposes, adaptation to changing circumstances" (p. 146). In 1838, before a decisive battle with the Zulus, Afrikaner commandos vowed that, if they were victorious, they would build a memorial church on the spot and future generations would celebrate the victory by honoring God. Ironically, in the years immediately following the battle the anniversary was rarely observed nor was the exact wording of the vow recorded. As time went on, however, the myth evolved, and December 16, a national holiday now called the Day of the Vow, became a chauvinistic annual celebration at which Afrikaners reaffirmed their sense of racial supremacy and fostered nationalist aspirations. In 1938 the celebration was nationwide and culminated in the laying of the foundation of the Voortrekker Monument in Pretoria. In many ways this event was a precursor of the National party electoral victory in 1948. Thompson shows how this myth has been interpreted and reinterpreted to emphasize God's partiality to the Afrikaners and, in particular, their sense of isolation in a hostile world. The myth of the covenant provides a justification for Afrikaner political actions by emphasizing race and group solidarity, and conservative Afrikaners use it today to validate their opposition to the Nationalist party government's limited race reforms.

In an ideal world, according to Thompson, a political mythology would satisfy three criteria: "It would conform to the evidence, it would be scientifically valid, and its effects would be wholly beneficial. Providing models of human personality and behavior, it would help humanity to profit from the achievements and avoid the errors of previous generations. In the real world, political mythologies fall short of satisfying one or more of these criteria, and many mythologies have nefarious effects" (p. 20). Thompson leaves the responsibility of discrediting "false and noxious myths" to the historian (p. 23).

Although Thompson provides the intellectual provenance for his criteria, he provides little indication of why he selected these and not others. His first criterion is particularly problematical. A myth that distorts the evidence certainly will fail the historians' test because, as Thompson observes, the role of the historian and that of mythmaker are incompatible. But he does not fully recognize that this does not diminish the effectiveness of the myth. Moreover, whether the effect of a myth is "good" or "bad" is in the end so relative that it becomes almost meaningless. Surprisingly, Thompson all but ignores the ideological dimension of apartheid. Indeed, the combination of the scientific and the mythological is what provides ideology with its ability to serve as a guide to reality. Although the mythology of apartheid may have been based on racial myths, the ideology of apartheid has always been subject to pragmatic considerations.

Thompson's work is a bold experiment. He forcefully makes us reassess the role of political mythology in history. But we must also avoid a mythocentric view of reality. In South Africa political myths have been a neglected area of study, but they seem to have dominated Afrikaner consciousness. Although the myths were important, Afrikaners also consciously manipulated organizations, literature, their churches, and other institutions. Nonetheless, this is a useful book because it points the way to a potentially important branch of scholarly research for South Africa and, indeed, other parts of the world. Of particular interest will be the ways in which those historians and sociologists who empha-

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size the differing perspectives of race and class in analyzing South Africa will receive this new approach.

PATRICK O'MEARA
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ASIA AND THE EAST

YEN-P'ING HAO. *The Commercial Revolution in Nineteenth-Century China: The Rise of Sino-Western Mercantile Capitalism*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xv, 394. $37.50.

The major contribution of this welcome book is Yen-p'ing Hao's careful and creative use of the business archives of some of the major British and American firms that traded in China. The result is a marvelously full and realistic account of Sino-foreign commerce on the coast of China from the 1820s to the 1880s. Of course, it was trade that first brought the foreigner to China and set in motion the confrontation that produced the system of unequal treaties that dominated China's external relations from the Opium War through World War II. And much of the early scholarship on China's modern history—H. B. Morse's magisterial volumes, for example—accordingly centered on commerce and its effects. But recent fashion has at times held study of the Sino-foreign commercial interaction in the coastal treaty ports to be unfashionable, if not entirely superficial.

Hao shows that recent fashion is quite mistaken through his research in the historical archives of Jardine, Matheson and Co., the premier British firm in China; of Butterfield and Swire, Jardine's great rival; and of the notable American merchants in the China trade, in particular Augustine Heard and Co. and Russell and Co. His enormously detailed exposition of the manifold day-to-day operations of these firms and of their Chinese counterparts (which, because their own records are lacking, must be seen largely as reflected in the foreigners' letters and account books) is skillfully tied to major questions about the China trade, which has not before been so thoroughly examined. The reader learns, among other things, about the size and the nature of the important expansion of credit in nineteenth-century China; about the centrality of the opium trade in the business of most major foreign firms until perhaps the 1870s; about the commercialization of agriculture (the main example is the tea trade); about Sino-foreign cooperation, and fierce competition in trade; about the large Chinese investment in foreign enterprises and the diversification of both Chinese and foreign traders into ancillary financial, shipping, and even industrial activities; and about the great market risks and the inevitable losses and bankruptcies of foreign and Chinese firms that accompanied their sometimes immense profits. Hao's footnotes are gems of apt quotation from his business records to illustrate all of these and other matters.

Less useful, I think—because they seem to respond to fashion's insistence that one be "relevant"—are Hao's introduction and conclusion. He really does not need these tepid discussions of "dependency theory," the "incipiency of capitalism," or "economic imperialism" to give meaning to his fine book. None of the interpretive issues dear to these old faithfuls of some historical discourse can ever actually be researched—as opposed to speculated about, sometimes quite usefully for heuristic purposes. The treatment here is too fragmented—tacked on, maybe—to be very helpful. In contrast, the missed opportunity to compare this mid-nineteenth-century commerce and its consequences with the more "routinized" or "institutionalized" trade of the twentieth century is a lacuna that Hao should not have left. I would guess that critical differences separate the still-swashbuckling foreign trader of this pioneering epoch from his more prosaic successors in a later age when foreign trade in China was less of a "zero sum game."

ALBERT FEUERWERKER
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ADRIAN A. BENNETT. *Missionary Journalist in China: Young J. Allen and His Magazines, 1860–1883*. Athens: University of Georgia Press. 1983. Pp. xii, 324.

THOMAS A. BRESLIN. *China, American Catholicism, and the Missionary*. University Park: Pennsylvania State University Press. 1980. Pp. 144.

The experience of American missionaries in China during the twelve turbulent decades before 1949 has created a rich historical record for students of the United States and China. Yet only in recent years have serious studies of American missionary activities in modern China begun to indicate the scope and depth of the missionaries' involvement in the processes of cultural and political change. Both of these studies treat aspects of this question.

Adrian A. Bennett's monograph focuses on the career of Young J. Allen, who abandoned life in Georgia as a southern aristocrat in 1859, selling an inherited plantation and slaves to pay for his family's passage to China, where he served for forty-five years as a Southern Methodist missionary. Using both Western and Chinese sources, this important study of Allen as preacher, educator, translator, and publicist of secular as well as religious materials meticulously documents the extent of American Protestant engagement with China's "self-strength-

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ening" and reform movements during the last decades of the nineteenth century. Bennett shows how Allen, armed with an education from Emory University and the conviction that Western science would help convert heathen China to Christianity, sought to use secular information to open Chinese eyes to the wonders of the West. After several frustrating years of preaching in and around Shanghai while employed as a teacher and translator by the Chinese government (which granted him official rank), Allen ventured into journalism as a means to reach the literate public with ideas aimed at undermining the Confucian establishment and numerous elements of Chinese culture.

Bennett demonstrates that Allen's participation in the launching of modern journalism in China with the publication of his *Chiao-hui hsin-pao* (Church News [1868–74]) and *Wan-kuo kung-pao* (Chinese Globe Magazine [1874–83]) offered revolutionary concepts for intellectuals open to ideas from beyond China's cultural boundaries. More than half of Bennett's book presents a fascinating examination of Allen's Chinese materials that introduced to readers basic elements of Christianity, a plethora of information on the non-Chinese world, and criticisms and suggestions for Chinese policies.

Some intriguing, if difficult, questions remain beyond the scope of the monograph, however. For example, how did Chinese readers interpret and use such alien thought and new information? How were Western concepts understood when they were translated into Chinese characters? Even though several Chinese reformers such as Wang T'ao and K'ang Yu-wei no doubt read Allen's magazines and were inspired by them, as Bennett admits, Allen's impact on China is "difficult to assess" (p. 235). It is a question that needs further analysis.

Few scholars have examined the three-decade history of American Catholic missions in China, the subject of Thomas A. Breslin's book. A short but illuminating survey of American Catholic missionary institutions in China from 1918 to their expulsion after 1949, Breslin's study describes how this missionary effort was doomed by the rise of Chinese nationalism and by missionaries' bungling. Urged into China by the Vatican to fill positions vacated by European Catholics after World War I, American Catholics generally did not enter this field with the enthusiasm of their Protestant compatriots. Breslin argues that the missions in China nevertheless were institutional safety valves for the American Catholic church, providing opportunities for surplus religious aspirants: these outposts were "dumping grounds for enthusiasts, mavericks, and misfits" (p. 112).

Unlike Allen, who focused his endeavors in an urban center, American Catholic missions scattered across rural China from Manchuria to Kwangsi, seeking converts before they were corrupted by the "sophistication and worldiness" of urban society (p. 107). Although American Catholics provided educational facilities, medical care, and relief aid to untold numbers of Chinese, the missionaries' success was undermined by their adoption of a life style that set them above most Chinese. In particular, Catholic missions as landlords gave the appearance of a church that, having come to do good, did very well, and thus provided targets for both Nationalist and Communist xenophobia. In some cases the American Catholic's response neither manifested an awareness that the missionary was a foreigner in a foreign land nor exhibited the qualities of Christian charity. For example, in 1930 a Vincentian wired the American consul following his mission's occupation by Nationalist troops: "Nanking Government lies, lies, lies, lies. Demand action. My residence worth $1,000 a day. Occupied since May. If you demand payment these yellow rats will get out" (p. 66).

Although Breslin's compact history does not ignore the American Catholic Church's impressive list of good works in China, it demonstrates how the endeavor fell prey not only to the unsettling events of war and revolution but also to its own policies and inadequacies, causing it to be viewed by modern Chinese leaders as a major component in China's "semicolonial" status. Based on several archives of representative missionary groups, this book is a welcome contribution as a preliminary to more detailed studies of individual American Catholic societies in China.

Most Chinese rejected both Protestant and Catholic religious appeals, but attempts such as Allen's to infuse the mind of modern China with new information, practices, and ideas combined in the end with Chinese perceptions of missionary behavior to leave a potent residue in contemporary China. Bennett's and Breslin's contributions make this heritage easier to appreciate.

FRED W. DRAKE
University of Massachusetts

WILLIAM WAYNE FARRIS. *Population, Disease, and Land in Early Japan, 645–900.* (Harvard-Yenching Institute Monograph Series, number 24.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1985. Pp. xvi, 235. $20.00.

During the period designated by the title of this work, Japan was first centralized under an elaborate bureaucratic system, only to begin a gradual process of political fragmentation. This retreat from institutional grandeur has become the focus of a formidable corpus of Japanese historical scholarship,

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whose complexity William Wayne Farris attempts to simplify by introducing a few simple hypotheses regarding epidemic disease and agricultural productivity. The norms applied de-emphasize organizational change within the rural society on whose tribute the imperial state depended. Farris proposes that the great smallpox epidemic of 735–37 ushered in two centuries of inevitable stagnation, as recurrent plagues and a low level of agricultural technology combined to prevent further progress.

The inability of the central government to maintain over the long term the rigorously authoritarian fundamental legal codes (*ritsuryō*), the best documented political feature of the eighth and ninth centuries, is not in itself a major preoccupation of this work. The basic eighth-century source materials relating to land policy and population registration are introduced, however, and analyzed with great skill and care. These discussions of people and land in Japan's classic age are major contributions to Western Japanology and should attract wide interest among scholars concerned with premodern populations. Never before has this material been presented so effectively in a Western language.

The major problem with this book is the author's attempt to fit his Japanese subject matter into a more general framework by insisting that his data confirm William H. McNeill's thesis that pandemic conditions inevitably bring about economic and social stagnation. Ferris argues that, in the century before the epidemic, when the *ritsuryō* state was built, the population was growing at a rate of about 1 percent per year. This estimate is derived from an analysis of surviving population registers from the early eighth century and a comparison of the resulting "age pyramids" with Cole-Demeny models. Despite the uncertainties of this procedure, a good case is made for the claim that the period of state building was one of population growth, as eighth-century officials seem themselves to have believed. After the great smallpox epidemic, however, Japan became inextricably linked to the Eurasian disease pool and was, like medieval England, doomed by its insularity to recurrent pandemics introduced from the neighboring mainland. The eradication of over 20 percent of the adult population and the depletion of official granaries brought the imperial government to a state of near bankruptcy, forcing it to condone the tax-farming schemes that ultimately undermined it. The sudden impairment of central authority, however, does not by itself add up to the centuries of social stagnation that McNeill's hypothesis seems to require. A closer examination of Japanese society in the centuries following the first great plague would seem to be a minimum prerequisite of such a sweeping assertion of enervation by pestilence.

Chapter 2, an impressive scholarly monograph in its own right, deals with the great epidemic itself. The rather sparse source materials are effectively marshaled, and two major source documents, beautifully translated. Convincingly presented, the evidence indicates that this horrendous event was Japan's first encounter with smallpox, and the translations clearly convey the alarm that accompanied offical efforts to control the disease. Listing all local and national epidemics reported from 698 to 898, Farris seeks out every link between this history and that of the Eurasian land mass. East Asia as a whole, he argues, was suffering from an influx of new diseases, resulting, as McNeill would maintain, in periodically "recurrent epidemics of heavy mortality" (p. 71) over the time span required for the infections to become endemic. In Japan the great smallpox epidemic was followed by repeated outbreaks about once every thirty years, until 1061. Eighth- and ninth-century records, although usually failing to identify diseases clearly, indicate severe visitations by other plagues entering from the continent. Smallpox was merely the most virulent of several contagions introduced during the period. The Japanese records cited do indeed seem to confirm McNeill's position regarding this point.

How these circumstances affected Japanese society in the long run is discussed mostly in relation to land use and agricultural productivity. Technology was far more rudimentary than it was to be in Edo times, and yields were much lower. Farris argues that, in the centuries immediately following the plague, improvement of any sort was impossible; McNeill's thesis of stagnation is, in effect, presumed true. This presumption leads to what is perhaps the most provocative assertion of the entire work: that the rise of a new propertied rural elite in the ninth century resulted not from greater surpluses but from increased scarcity. He asserts that the plagues and famines of the period depleted the resources of the state more seriously than those of the local gentry, who then seized control over the beleaguered peasantry. The documentary evidence presented to show how low rice yields were is neither as unambiguous nor as trustworthy as it is made to appear. Higher land assessments, reporting officials knew, led to increased tax liabilities. This sort of bias is ignored. Farris's valuable discussions of land laws and local surveys sometimes appear to be mere embellishments for a predetermined conclusion.

This work nevertheless deserves high praise for its informative discussions of rural technology in classical Japan. Of particular interest is the failure of certain Chinese techniques, such as the use of water wheels, to take root despite official promotion. Artifical irrigation of all sorts, Farris argues, occupied a relatively minor place in the peasant economy. The generally accepted version of the farmer

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and his relation to the central government in the *ritsuryō* period has been critically challenged.

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JOHN M. FRITZ *et al.*, editors. *Where Kings and Gods Meet: The Royal Centre at Vijayanagara, India.* Foreword by BURTON STEIN. Tucson: University of Arizona Press. 1984. Pp. 158. $28.95.

Vijayanagara, the "city of victory," was the capital of the last great Hindu empire of South India. Founded in the mid-fourteenth century, it rapidly became one of the largest and wealthiest cities in the subcontinent. The power and magnificence of its rulers were the envy of the Muslim sultans and the lesser Hindu kings. The reputation of the city reached even Europe, and several foreign visitors left vivid accounts of life in the capital. In 1565 the city was sacked by Muslim armies and abandoned.

Vijayanagara is the focus of this work, which has as its "central theme" that the meeting of king and god is most clearly manifest in one part of the city—the royal center. "It was here," John M. Fritz *et al.* assert, "that the public and private activities of the kings were mainly focused. Our documentation project concentrates on this zone and aims at recording the form and spatial organization of all visible features" (pp. 5–6). The authors employ two sets of archaeological techniques in the study of the material record of Vijayanagara: excavation and surface archaeology.

The second chapter, "Elements of Urbanism," describes various zones of the city: the "sacred center," the urban core, suburban centers, intermediate valley, Anegondi, and roads. The next chapter contains a detailed description of the royal center, especially its architectural and archaeological features that include the original fortification walls. Various configurations of the thirty-one enclosures are also located and described. The hundred-columned hall, Ramachandra temple, the so-called zenana, and the great platform seem to have been selected for emphasis. Chapter 4 describes boundaries and routes, walls, gateways, roads, and waterworks described with line drawings and a road map (p. 49), which highlights the northeast road and its numerous shrines as well as the junction near the "king's palace" and the Ramachandra temple.

Chapter 5, the largest in the book, focuses on the sacred art of the temples and shrines. Vijayanagara contained a large number of Hindu and three Jaina temples (perhaps even some mosques). The two Hindu temples, Vaishnava Ramachandra and Shaiva Virupkaksha, command special attention (p. 59). Next, the authors detail platforms and palaces in the subsequent chapter, but they do not seem to agree with earlier identifications of important buildings, for example, the "king's palace" and the zenana. Islamic-styled forms—towers, pavilions, and stables—are discussed in chapter 7, attesting to a significant self-selected Islamic influence on Vijayanagara. The most impressive building in this category appears to be the elephant stables. Finally, themes of interpretation are presented briefly with a "tentative" conclusion in the last chapter.

On the whole, this is an excellent and exhaustive report. It contains many fine line drawings and maps; strangely, no coins are mentioned. Although the authors do mention other historical, epigraphical, literary, and travel accounts of Vijayanagara, they fail to use them for presenting a fuller portrait of this glorious city. They overemphasize the "meetings of kings and gods" but are not clear even about the meetings of the kings, courtiers, queens, princes, princesses, and subjects. Nor are they sure of the precise functions of different important buildings in the royal center. Nevertheless, handsomely bound and neatly printed, this work represents a major contribution to the study of Vijayanagara through international collaboration and financial support.

JAGDISH P. SHARMA
University of Hawaii

OM PRAKASH. *The Dutch East India Company and the Economy of Bengal, 1630–1720.* Princeton: Princeton University Press. 1985. Pp. xii, 291. $38.50.

AHSAN JAN QAISAR. *The Indian Response to European Technology and Culture (A.D. 1498–1707).* New York: Oxford University Press. 1982. Pp. xiii, 225. $35.00.

Both these works deal with Mughal India. Ahsan Jan Qaisar's thesis is that Indian society had no built-in resistance to change. He looks at shipbuilding, land transport, military technology, printing presses, mechanical clocks, mining, textile manufacture, and the arts. One wonders if such topics suitably expanded could make monographs in themselves. Certainly his comments on painting, architecture, and music could stand alone. In his view the Mughal school of painting was originally Indo-Persian but seems to have been influenced in its later phases more by European art than Persian. Akbar's interest in the exotic was one of the most influential factors in its development, and Jahangir and Shah Jehan were greatly interested in European painting. Many of the churches, fortresses, and tombs reflected European design, although European music was not adopted to any extent.

Om Prakash has produced an impressive study drawn from, among other sources, previously unexploited Dutch archives. He is concerned with the economic activities of the Dutch East India Company in Bengal. Both the Dutch and the En-

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glish came to India to trade, but the British company became the nucleus of an enormous colony, whereas the Dutch, eschewing settlement, carried on the greater volume of trade. The most marked feature of Dutch activity was extensive intra-Asian commerce (the country trade). Profits from this intercourse helped the Dutch dominate the commerce with Europe through the greater part of the seventeenth century. From the Persian Gulf to Japan they operated as a single agency. The most important product that they moved was rough cotton textile from the east coast, and Bengal was the seat of their activity. Bengal raw silk became the chief export to Japan, and Bengal opium found a widening market in Indonesia. The spice monopoly in Indonesia together with access to the "Hermit Kingdom" of Japan made for highly profitable intra-Asian commerce. When intra-Asian trade declined, Bengal became a major supplier of goods to Holland.

Both the Dutch and the English were exempt from the payment of transit duties throughout India. They thrived and their extensive trade brought precious metal into the empire. The principal beneficiaries of this trade were the Indian merchants who supplied export goods to the companies, although Indians engaged in coastal and overseas trade also benefited. The chief advantage of the Indian merchant was his low overhead and familiarity with the markets both home and abroad.

By 1720 the Dutch company had to cope with growing competition from the British and the dishonesty of its own employees. In volume of trade the Dutch company was still the most important trading body in Bengal. Not until decades later did the British outstrip the Dutch, and this happened to a great extent because of the political and military victories of the British over the Mughal.

The social effect of Asian and European trade is not covered by Prakash's study. The impact of European technology and culture is, of course, the subject of Qaisar's investigation. He contends that Indians picked up European technology out of convenience, utility, or other pragmatic considerations, and there was no conservative pressure to resist. Despite much detail, his book remains somewhat superficial, and the style is occasionally clumsy. Prakash's research, on the other hand, bites much deeper. It roughly parallels Holden Furber's classic *John Company at Work* (1948) and is destined to become its companion piece.

MARK NAIDIS
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B. R. NANDA. *Gandhi and His Critics*. New York: Oxford University Press. 1985. Pp. ix, 178. $17.95.

After being largely ignored in the 1970s, the life and thought of Mahatma Gandhi has become a topic of increasing interest in the 1980s, much of it focused on the Gandhi portrayed in Richard Attenborough's film. Since Gandhi has always attracted controversy and, as B. R. Nanda points out in his preface, "was continually in the centre of one storm or other," the film has awakened not only adulation but also criticism. Nanda, who has written successful biographies of Gandhi, the Nehrus, and Gokhale, takes on these latter-day critics and, in refuting their attacks on Gandhi, redefines Gandhi's importance to a new generation that may not be aware of how germane his ideas are to today's problems.

The recent criticisms of Gandhi have been heard and discussed repeatedly, and it is surprising that they have been resurrected. Critics claim, for example, that Gandhi defended the caste system, that he ignored the grievances of the blacks in South Africa and helped only his own community, that his struggle for Indian independence was unnecessary because the British had already decided to give India its independence, and that the British were so decent and humane that they were easy victims of nonviolent methods. Critics assert, moreover, that Gandhi's use of Hindu symbols and his emphasis on religion alienated the Muslims and eventually led to the partition of India and the ensuing bloodshed. Allegedly, he was hypocritical about the use of violence and supported the British war effort in the Boer War and World War I. His economic ideas were supposedly primitive, naive, and harmful to India, and he was friendly to the exploiting capitalist class. Finally, the critics claim that his celibacy was not only unfair to his wife but also hypocritical because he shared his cot with young girls.

The latter-day critics are lightweights, and Nanda has no trouble refuting them and presenting his own balanced and reasonable interpretation of Gandhi. The book is disappointing, however, because he did not contend with such serious critics and adversaries of Gandhi as Rabindranath Tagore, Subhas Chandra Rose, Martin Buber, B. R. Ambedkar, M. N. Roy, Jawaharlal Nehru, and Erik Erikson. An analysis of their differences with Gandhi would have taken us beyond the realm of conflict over "facts" and into debates over fundamental philosophical issues.

Nevertheless, one must review the book Nanda did write rather than the one he might have written. He aimed this book at the general public, not at academics, and has written in his customarily natural, simple, and clear prose about a Gandhi who is straightforward, reasonable, and guileless. Nanda's strongest chapters are those that deal with Gandhi and the raj. In answering assertions that, aside from a few lapses such as the massacre at Amritsar (1919), the British were benign rulers who governed with

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the welfare of India at heart, Nanda points out that, like any other colonial regime, their rule was based on coercion and violence and that the British Indian empire was an unnatural and immoral institution. Nanda argues that the British would not have given up India without a fight and that Gandhi's tactics were primarily responsible for giving the British an opportunity to depart gracefully.

His weakest chapters are those on communalism and partition. This may be because, as a biographer, Nanda tends to place too much importance on individual actors and not enough on complex historical processes. In his discussion of the partition of India and its bloody aftermath he compares the behavior of Gandhi with that of Jinnah, to whom he assigns the primary responsibility for partition. But could either man have reversed the powerful tide of Islamic nationalism in India?

Nanda concludes that Gandhi is particularly relevant for this generation because of the threat of mass destruction and that, in the struggle between Gandhi's soul-force and Freud's death wish, nothing less than the survival of mankind is at stake. Nanda is ideally equipped to reinterpret Gandhi for our age, and performing that service is perhaps more important than writing a more original work for specialists.

BLAIR B. KLING
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BRIAN H. FLETCHER. *Ralph Darling: A Governor Maligned*. New York: Oxford University Press. 1984. Pp. xxi, 473. $37.50.

The governors of Britain's overseas empire in the first half of the nineteenth century found themselves almost universally faced with painful and often well-nigh intractable challenges. The view from Government House was rarely serene or attractive. Few historians with any detailed knowledge of imperial administration find it possible now to assess individual governors simply as autocratic tyrants or as impeccable heroes. As the empire expanded, so in principle did the responsibilities of the bureaucracy in Downing Street. Yet so, also, did the implausibility of carrying out the task in London. For the Australian colonies, as elsewhere, intermediary agencies that were local focuses of authority and of the colonial civil services were needed, as were the creation and development of subdepartments that could implement "policy" where necessary and more broadly superintend the routine workings of colonial administration on the spot. The colonial governors and their officials filled this role. The expansion of settlement, governed as much or more by local initiative on the periphery as by imperial plan or fiat, contained within its rhythm a deepening of experience in the colonization process and called for increasing sophistication in the setting up of working patterns and structures of government and administration.

During the 1820s much was accomplished. For the Australian colonies the reports of John Thomas Bigge's Commission of Enquiry (1821–24) were a watershed. To be effective, power had to be shared with a wider circle. The earlier convict society of Australia had been a colonial "hybrid" but it now became clear that autocratic government could only be maintained in a static "slave" society and that the course of free settlement in the Antipodes was bringing about an irreversible metamorphosis. No governor was more caught up in this inexorable but largely uncharted process than was General Sir Ralph Darling, governor of New South Wales from 1825 to 1831. Brian H. Fletcher's splendid new biography joins Hazel King's *Richard Bourke* (1971) and A. G. L. Shaw's *Sir George Arthur* (1980) to round out a mature and detailed understanding of this key period of Australian development.

The appointment of governors was an important function of imperial administration. In the days of autocratic rule the governor exercised immense powers locally, and in any case the vast distance between England and Australia thrust responsibility on to the colonial authorities. Much of the characteristic style of each colony derived from the energies, judgment, and vision of the governor, who became local monarch, prime minister, and chief executive. The transition from military autocracy to a more "liberal" regime, in hindsight inevitable, seemed startling to contemporaries and gave birth to myths of "heroes and villains." In fact, almost all the early colonial governors served Australia remarkably well within the limits of their time and talents. It is one of the merits of Fletcher's study to redeem the hitherto-maligned Darling from his reputation as a narrow-minded, limited military tyrant making petty scandals at the utmost ends of the far-flung empire. The book places Darling successfully in the broader context of his humble birth, brave fighting career in the Peninsular War, able service in the War Office, and amiable private life. Fletcher's meticulous research has enabled him to illuminate widely diverse themes and issues with admirable common sense and sureness of touch. The bulk of the book concerns Darling's work in Mauritius and especially in New South Wales. The treatment is leisurely, judicious, and satisfying, and the material is always interesting. Much was to be established, much to be done. Darling's six years in New South Wales may have been full of frustration and disillusionment, but he also made solid achievements and laid a foundation for his attractive successor, Richard Bourke.

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Fletcher's study is an accurate guide to the period and the issues involved. The book is beautifully produced. It significantly enlarges the horizons of Australian and imperial history.

J. J. EDDY
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UNITED STATES

C. VANN WOODWARD. *Thinking Back: The Perils of Writing History*. Baton Rouge: Louisiana State University Press. 1986. Pp. x, 158. $12.95.

Publishing historians who have survived the half-century mark in their careers have looked back on their work with feelings of satisfaction and the emotions accompanying the trauma of being revised. Over the past half-century, rising generations of historians have pushed the frontiers of research and interpretation of southern history well beyond the expectations of the 1920s. In this introspective book C. Vann Woodward has reviewed his own works with philosophical objectivity. In his long career he has seen the changes and shifting interests and emphases that have occurred in his field.

Three chapters of this book were delivered as the Walter Lynwood Lectures at Louisiana State University. That university press has published a more extensive text than it has done for previous volumes in this series. The author looks back on a half-century of prime historical scholarship, reviewing with candor the strengths and weaknesses of his books. It seems only a fortnight ago that a promising young scholar just out of graduate school published his *Tom Watson: Agrarian Rebel* (1938). This was an impressive book from the standpoints of both imaginative writing style and the presentation and interpretation of a complex and contradictory subject.

Woodward's scholarly career was in a manner disrupted by World War II, but not completely. During his naval career he produced three service books that no doubt honed both his writing and research capabilities. Back in academic life he took a diversionary tack in the research and writing of *Reunion and Reaction: The Compromise and the End of Reconstruction* (1951). His main objective at this point in his career was the writing of the *Origins of the New South* (1951), one of the ten-volume History of the South Series. When this book was published, it set a major milestone in the presentation of the emerging modern South as a region undergoing tremendous internal struggles and changes. The era that Woodward covered was neither dull nor vacuous as some scholars believed. Powerful forces that may well have fueled the debate about continuity and discontinuity were at work. Woodward denies having attempted "to announce or frame a thesis" (p. 78).

The essay describing the research, writing, and literary history of the *Strange Career of Jim Crow* (1955) gives a keen insight into an era of badly mixed and tragic racial relations at both the national and regional level. Whatever revisions in southern history may now have been made or will be made in the future, Woodward's insight and judicious literary skill in defining the central facts and forces must stand as a seminal indicator of new directions. Obviously, no author writing so close to the unfolding of the emotional events of the three decades from 1940–70 could hope to escape revision and criticism, but Woodward made an impressive start in delineating dynamics of this troubled era. In his formative years he had opportunities to sense first-hand the cross-currents that were rapidly transforming the South.

This surely is not a scholar's valedictory. The essays, however, reflect arrival at a stage where a historian can look back with considerable objectivity, charity, and some humor on a career set apart from those of most historians. Some of the perils of writing history and of an extended career must be seeing earlier works revised and updated by new research and broadened perspectives. Nevertheless, the historian has to start somewhere, and Woodward made some impressive starts in key areas of the history of the South.

No matter how many fresh discoveries and published works may appear in the future, Woodward's writings will have to be considered solid demonstrations of the state of historical craftsmanship for his era. At the outset of the present book he recognizes the price of writing with seasoned conviction and the passions inherent in controversy.

THOMAS D. CLARK
Lexington, Kentucky

V. A. TISHKOV. *Istoriia i istoriki v SShA* [History and Historians in the USA]. Moscow: Nauka. 1985. Pp. 351. 2 r. 60 k.

This book is a predictable mixture of straightforward information and tendentious commentary. The introduction declares that "history in the United States fulfills an active ideological mission, for the power of the possessing classes is purposefully used to justify and extol the capitalist order and the bourgeois way of life" (p. 5). The book itself contains much evidence that nothing of the sort is true for a large share of American historians. Fortunately, the largest part of the text is devoted not to argumentation of that kind but rather to factual description of the U.S. historical profession, archives and libraries, the organizations and journals

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of the American Historical Association (AHA) and the Organization of American Historians (OAH), and other societies. Noting that several other Soviet historians have written on the development of American historical thought, methods, and schools, V. A. Tishkov claims to present the first rounded picture of the place of history in U.S. higher education, the preparation of professional historians, and actions and ideas characteristic of their associations. His sources are the records and archives of the AHA and OAH, surveys taken of historians in America, the personal papers of certain individuals, and interviews with fifty-three historians.

In large part the contents of the book will not surprise American historians; what is often of interest is what strikes the author as surprising or noteworthy. For example, the report that 130 million people watched "Roots" on television earns a rare exclamation point. Tishkov casually states that earning a B.A. takes four years, an M.A. one year, a Ph.D. three years more; many doctoral students wish it took no longer. The flexibility of the American system will doubtless sound attractive to many Soviet readers, and Tishkov's admission that it "stimulates the independence of students" might serve to nudge those who keep Soviet higher education in lockstep. He does note that students specializing in history increased over the last decade only in Vermont, which may be news to many U.S. readers, but should not seem startling.

As usual in Soviet books, footnotes in Latin script contain many errors: Baylin should be Bailyn; Brindenbough should be Bridenbaugh; Cunliff should be Cunliffe; Fais should be Feis; Friedel should be Freidel. The Progressive candidate in 1948 was Henry, not George, Wallace. But in general the book is carefully proofread.

The author devotes ample attention to the activities of the Left: the few communists and the more numerous radicals and revisionists. Tishkov does not take kindly to criticisms of the USSR, and his responses are often quirky. Arthur Schlesinger protested treatment of Andrei Amalrik; that Amalrik "secured no work as a historian in the United States" after he emigrated is adduced as undermining the basis for Schlesinger's concern (p. 282). Enumerating the smaller societies of historians, the author fairly splutters when he comes to the Seventh-Day Adventists: "It is impossible to imagine how one can unite the process of gaining scholarly knowledge and the contemporary level of professional historical research with religious dogmas, especially the obscurantist postulates of the Adventists" (p. 292).

Tishkov must, of course, maintain the position he has set forth earlier that U.S. historians are narrowly bound to the class struggle. But he quotes with enthusiastic approval the antinuclear resolution of the AHA of 1982, adopted on the heels of debates recalling the stormy (but heroic, he implies) sessions of the 1960s and early 1970s. Quoting criticisms of U.S. foreign policy made by the late Ray Billington in letters to his former student, Senator George McGovern, Tishkov asks rhetorically whether Billington is a rarity in so speaking, and replies, no, "in private correspondence and conversations very many historians express comparable attitudes and views" (p. 347).

After paying ample attention to radicals, Tishkov ends with a sort of honor roll of twelve leftists. Despite all this evidence of anti-Reaganite sentiment, he concludes that the American historical profession remains "an essential element in the ideological mechanism of the contemporary state-monopoly capitalism of the U.S.A." (p. 349).

The book may serve to provide extensive new information about American historians and their profession to many Soviet scholars and students, who are accustomed to ignoring or discounting the ideological verbiage that such books must employ. The evaluative portions will sometimes annoy, sometimes amuse American readers, who will be few in number.

DONALD W. TREADGOLD
University of Washington

ROBERT H. WALKER. *Reform in America: The Continuing Frontier*. Lexington: University Press of Kentucky. 1985. Pp. ix, 270. $25.00.

Returning to the central theme of his earlier anthology of American reform, Robert H. Walker here refines his perception of reform as a continuous process of cultural self-definition. Although he acknowledges gracefully the contributions of scholars who emphasize the distinctiveness of separate movements and eras, he contends that historians generally have failed to appreciate the "extent and cohesion of directed social change" and thus have "undervalued" a critical aspect of the nation's history and character (p. 186). Reform is a "continuing frontier," an appreciation of which can best be attained through the study of "process," including the continuities in social change as well as the conflicts and cycles.

Walker identifies three main "modes" of reform in the American experience that converge in a national commitment to social meliorism. Mode 1 is political and economic reform, here epitomized in the running debate from 1832 to 1913 over the "money question." Mode 2 is the persistent struggle for social justice for all Americans, particularly those outside the mainstream, exemplified in the movements to define and defend civil liberties, extend civil rights to blacks, and liberate women. Mode 3 is the search for a better society through the planning,

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advocacy, and implementation of alternative models such as communes, utopias, and world governments. The geographical and chronological diversity among the three modes, plus the number and variety of individuals and groups identified with reform throughout American history, constitute for Walker a staggering commitment of time and energy—enough, surely, to give reform a central place in the national experience; enough, perhaps, to outweigh traditional competitive individualism with a demonstrated concern for the commonweal.

At the very least, the numerous and varied parts of reform assemble to form a coherent, dynamic force, whose influence is evident in all aspects of national development, from the settlement of the West to the war on poverty. Although Walker gives scant attention to foreign policy, his few words on the reformist roots of the League of Nations suggest that his reform tradition typically follows the flag as well.

Have historians neglected reform as a continuum and integral part in the American experience? Not in this reviewer's perception. Indeed, it is difficult to think of a major American historian who has not paid homage, in one fashion or another, to the persistence of social meliorism—cyclic or otherwise—from the colonial period to the present. To be sure, most have focused on specific movements, but few would argue against a tradition of reform or see their movements as nothing but isolated fragments. And the more sweeping treatments of reform—those of Ralph Henry Gabriel, Merle Curti, Vernon L. Parrington, Stow Persons, Richard Hofstadter, and the Arthur Schlesingers senior and junior, for example—leave little doubt of the centrality of reform in American history. Social meliorism is indelibly associated with optimism, and optimism is the hallmark of most American historical writing.

Walker's contribution is addressing the subject of reform in a thoroughly systematic manner. The jargon of "modes" aside, his book offers a perception of reform that may stimulate other scholars better to integrate specific movements and eras into a "reform tradition." If he overstates the case for continuity as against conflict and contention, he nonetheless provides a refreshing framework for associating reform with what most Americans think of as their traditional values.

JOHN G. SPROAT
University of South Carolina

BENJAMIN B. RINGER. *"We the People" and Others: Duality and America's Treatment of Its Racial Minorities.* Reprint. New York: Tavistock. 1983. Pp. xii, 1165. $25.00.

This work by Benjamin B. Ringer, a sociologist who combines the methodology and vocabulary of his discipline with the data and perspective of the historian, is a major contribution to the scholarly literature devoted to ethnic and race relations in the United States. Based on prodigious research in court records, government documents (especially *The Congressional Record*), and scores of secondary works in diverse fields, which are quoted too frequently and too generously, this book is as broad in chronological scope (from the settlement of Jamestown to the present) as it is painstakingly, and on occasion painfully, detailed in its treatment of selected aspects of America's encounter with ethnic and minority groups. The volume is organized into three sections: the first introduces and explains Ringer's concept of duality; the second and third, comprising almost eight hundred pages, describe and analyze the impact of America's coexistent and contradictory societal models on Afro-Americans, Chinese-Americans, Japanese-Americans, and Puerto Ricans. Regardless of whether this volume deserves to be described as a magnum opus in the usual sense, it is indisputably a big book, one that weighs four pounds in paperback.

Ringer disputes the disparate interpretations of American ethnic and race relations espoused by the "orthodox assimilationist American creed scholars" represented by Milton Gordon and by the "Third World" school. At best they provide only partial explanations. His interpretation rests on the idea that America's historic treatment of its racial minorities has been both an expression and a product of the dialectical tension and struggle between the domain of the people ("We the People") for whites and the plural society for nonwhites. This duality is a legacy of the twofold processes of colonization and colonialization bequeathed by the European conquest and settlement of the New World. In the colonist society, especially in Virginia, membership in the people's domain was restricted to whites who shared various rights and immunities, whereas nonwhites, especially blacks and Indians, were consigned to a colonialist model of a plural society that "stressed the separateness of the races and the inferiority of the nonwhite" (p. 8). Contrary to what some authorities suggest, Ringer argues that the American experience with racial minorities has not been unique. A similar duality exists wherever Europeans were both colonists and colonialists.

From its beginnings the United States embraced what Ringer terms a Manichean dualism: on a higher, more visible level the colonist society of white America was transformed into a nation-state emphasizing the rights and sovereignty of the people and regulated by the American creed; on a lower, less visible level the founding fathers legitimated the societal model of a racially bifurcated

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colonialist America. Once ensconced in the Constitution, this duality, which was always a national rather than a regional phenomenon, was perpetuated by presidents, Congress, the courts, and bureaucrats for almost two hundred years. Although Congress transformed the racial creed from a legitimate to an illegitimate code of behavior in the 1960s, Ringer does not subscribe to the notion of the declining significance of race, especially as propounded by W. J. Wilson. In fact, he provides a lengthy critique of Wilson's hypothesis and concludes that race, rather than declining as a factor affecting the economic fate of middle-class blacks, has merely undergone a transformation.

Rejecting the optimistic projections regarding ethnic and race relations of those enamored of the power of the American creed, Ringer nonetheless concludes that the United States came closer in the 1960s to resolving its historic duality "than had any of the other 'colonist-colonialist' societies in which the white European had become the numerical majority" (p. 1101). But the resolution is far from complete. Unless the nation rids itself of the inequities that the duality of the past has so deeply engraved in present institutional arrangements, Ringer suggests the possibility of renewed racial disturbances.

This book complements and enlarges on Gunnar Myrdal's *American Dilemma*. Considerably broader in scope than this forty-year-old classic, Ringer's work is often most incisive in its treatment of ethnic and racial minorities other than Afro-Americans.

WILLARD B. GATEWOOD, JR.
University of Arkansas

FRANCIS PAUL PRUCHA. *The Indians in American Society: From the Revolutionary War to the Present*. (Quantum.) Berkeley and Los Angeles: University of California Press. 1985. Pp. ix, 127. $15.95.

Following the publication of his monumental work *The Great Father: The United States Government and the American Indians* (1984), Francis Paul Prucha now gives us a brief survey of the relations between American Indians and the United States government since the Revolutionary War. In four brief essays, originally presented as the Gasson Lectures at Boston College, Prucha provides a narrative history of government-Indian relations, asserts a point of view about Indian history, and comments on the work of many other major historians of Indian-white relations. Prucha finds four significant themes in the history of Indian–United States relations: paternalism, dependency, Indian rights, and self-determination. His first two essays, entitled "Paternalism" and "Dependency," cover Indian relations with the United States to about 1920. The United States always assumed a paternalistic attitude toward the aboriginal peoples of America, exemplified in the expression "the Great Father," used by Indians and whites alike to refer to the president during the nineteenth century. This paternalism seemed justified by the Indian's dependency, ironically produced by the presence of European trade, technology, and settlement in the New World.

In the last two essays, "Indian Rights" and "Self-Determination," Prucha examines the sixty years since 1920. A new attitude toward Indians, based on the insights of modern social science and the newly acceptable doctrine of cultural pluralism, led to defenses of Indian rights and a call for tribal self-determination. By the 1970s the federal government's Indian policy seemed very different from that of the nineteenth century. Rather than promoting the assimilation of Indian people, however, government assistance, which increased dramatically during the 1960s and 1970s, fostered the self-determination of Indian communities. In the absence of resources to provide self-sufficiency, continuing dependence on federal assistance threatened a return to paternalism in Indian relations.

Prucha sets out to present "some themes and patterns that might make sense to the non-specialist without being completely useless to scholars in the field" (p. vii). He succeeds admirably. This is a short book that can be read with profit as an introduction to Indian-government relations by a beginning student. Specialists, although they may disagree with some of the particular judgments that he makes, will appreciate the breadth and scope of Prucha's work. The writing is excellent. The footnotes demonstrate a prodigious amount of scholarship. Cited are Prucha's many earlier works on Indian-white relations, including *The Great Father*, as well as many of the major writings of other historians. These lectures stand as a summary of Prucha's work on United States Indian policy; they demonstrate the significance of that work well.

PAUL STUART
University of Wisconsin, Eau Claire

VIVIANA A. ZELIZER. *Pricing the Priceless Child: The Changing Social Value of Children*. New York: Basic. 1985. Pp. x, 277. $18.95.

When a sociologist declares that a major purpose of her book is correcting an erroneous and simplistic position of modern economists regarding how the marketplace functions, few historians would normally consider the book worthy of attention. But Viviana A. Zelizer's book is an excellent addition to the growing number of studies on childhood to follow the groundbreaking work of Phillippe Aries's

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Centuries of Childhood (1962). Zelizer argues that economists have failed to understand the multifaceted interactions that actually determine market values. To demonstrate her position, she undertakes a study of shifting American views toward the worth of children from 1870 to the 1930s. Her thesis is that, whereas in the nineteenth century children were primarily valued for their economic contributions, by the first quarter of the twentieth century sentimental value had so replaced instrumental worth that any hint of commercialism was abhorrent to the leaders of the "sacralization" movement and to the public in general. According to the author, the conflict between the older and the newer attitudes can be clearly seen in the areas of child labor, adoption, insurance, and legal stances regarding accidents and wrongful deaths involving children. Drawing on secondary and primary sources, she traces each of these topics and offers fascinating examples of the interaction between cultural values and the marketplace. I would recommend the book to anyone interested in social history, economic history, even history in general.

There are, however, some problems with the book. Zelizer argues that the urban middle class made the switch from an economic to a sentimental valuation by the mid-nineteenth century, as evidenced by the use of insurance, not children's labor, to support aged parents. But the child remained a commodity for the working class. Thus, the book really focuses on the disappearance of differences between the classes as child labor laws, compulsory education, and the sentimental value of children were imposed on and accepted by the working-class family. There is, however, no real explanation of how the middle-class position was changed. It would be informative to know whether the "priceless" child emerged as a middle-class concept and companion to the nonworking wife or rather percolated down from the upper class. One wonders whether Zelizer found any foreign influences or whether she sees this concept of the child as a truly American phenomenon.

Meaningful distinctions based on ethnicity and in some cases religion are not present. Chinese, Jewish, Irish Catholic, and native rural Protestant views of children are lumped together for most of the book. One brief comment that Jewish families in the twentieth century still preferred to adopt boys when everyone else wanted girls is offered in a tone of surprise. The book would be strengthened if more time were devoted to the differences between groups. The author does a better job acknowledging that rural Americans clung to the old views of child labor well into the twentieth century, and she does point out that a wide spectrum of American families returned to the older view during the late 1920s and 1930s. Rural America is never defined as either farm families or the farm-belt states, which include urban middle-class Americans. Also surprising is the absence of the role of labor unions in shaping attitudes about child labor. The American Federation of Teachers is mentioned once, and more radical groups, who broke ground in social and economic matters, are ignored. The label "progressive" is used to cover a group of political and social reformers that is much too broad. Finally, the author, intrigued by the legal need to price the priceless, moves well beyond the 1930s but does not explore this later period with sufficient detail.

Whether her disagreement with the economists is viewed as a convenient starting point or a serious debate does not really matter. Zelizer's book is well written and provocative, and it should spawn numerous studies to test and refine her thesis on the transformation of children from an economic to a sentimental and priceless commodity. One might even be tempted to suggest that future historians will consider the shifting position of work as the sole determinant of worth among adult Americans.

MIRIAM Z. LANGSAM
Indiana University-Purdue University, Indianapolis

WALTER L. CREESE. *The Crowning of the American Landscape: Eight Great Spaces and Their Buildings*. Princeton: Princeton University Press. 1985. Pp. xi, 289. $55.00.

This thoughtful, scholarly, and beautifully illustrated book is a history of eight places in the United States where human effort enhanced natural beauty to create the central sites in the American landscape tradition. The book is both an analysis and a celebration of these achievements.

Walter L. Creese is an architectural historian who combines a deep knowledge of the literary and philosophical sources of American culture with a remarkable talent for defining in words the special meaning of space. Much of the book is devoted to eloquent descriptions and precise visual analyses of each site, descriptions that benefit from Creese's diligent research in the primary sources and from the numerous well-chosen illustrations that accompany the text. But Creese is also concerned with two larger issues that help unify the book.

His first aim is to demonstrate the existence of an American landscape tradition, a common set of beliefs, techniques, and goals that tie the work of Thomas Jefferson to that of such distinguished successors as Andrew Jackson Downing, Frederick Law Olmsted, and Frank Lloyd Wright. The second aim is the complex and contradictory relationship between these "place-makers," as Creese calls them, and the rest of American society. All of them at-

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tempted to express in space the ideals of American culture, yet their plans did not flow naturally out of the pattern of American settlement. Their projects required time and patience in a society dedicated to immediate profit, a command of both technical and aesthetic skills, and a comprehensive vision of the public good. As Creese recognizes, the "crowning of the American landscape" was in conflict with the real forces of American society.

The book begins with Creese's archetype of American space, Charlottesville, Virginia, and of the American place-maker, Thomas Jefferson. Jefferson's true "great space," Creese argues, is not only Monticello and the University of Virginia but the whole region understood in Jefferson's terms as an ideal American environment: agrarian but truly civilized, "postulating," as Creese puts it, "better intervals among farms, towns, water, trees, people, and hilltops" (p. 41).

He next considers the Hudson Valley in the nineteenth century where, in Downing's phrase, "bright villas deep-set in verdure" inspired both the American picturesque and the American domestic tradition.

Creese's discussion of Yosemite and Mt. Hood National Forest with its WPA-built Timberline Lodge introduces the grandest natural spaces in the book but, in spite of the magnificent illustrations, these are disappointing chapters. Beyond thoughtful preservation, human efforts can contribute little to the value of these sites; the balance between human creativity and nature that forms the basic theme of the book is largely absent here.

This balance is at the heart of Creese's illuminating treatment of the Boston Fens. This section of the Back Bay was a polluted mud flat until Frederick Law Olmsted, working with an enlightened city administration, turned it into both a salubrious part of the city's water system and a carefully simulated piece of New England seashore landscape introduced into the heart of the city. Here imagination and technical expertise, a farseeing individual and a concerned society, united in a space that was at once a deeply personal work of art and an integral part of urban Boston. Yet the Fens project was no sooner completed than various insensitive "improvements" began to destroy it.

After a brief chapter on Graceland Cemetery in Chicago, Creese turns to another Olmsted design, the Chicago suburb of Riverside and its development from 1870 to 1910. He takes seriously the ambitions and achievements of this "greatest American suburb," as he calls it. It provided, he argues, a "possible consensus of how to live in a limited green space" (p. 240).

Creese's aesthetic and social concerns reach a fitting climax in his concluding chapter on Frank Lloyd Wright's Taliesin. For Wright took up the Jeffersonian challenge to be the prophetic interpreter of the American environment only to become increasingly alienated from American society. As Creese shows, the integration of house and landscape that Wright achieved in the Wisconsin farm valley where he was raised represents a true summation of the American landscape tradition, yet the perfection of Taliesin only emphasized the growing dichotomy between its principles and the realities of American life.

For Wright the only possible successor to Taliesin was Broadacre City, his utopian plan for a decentralized America, but this ultimate "great space" could only be realized in the wall-sized wooden model that Wright and his apprentices constructed in the 1930s. Appropriately enough, this model, which now hangs in the Hillside Home Gallery at Taliesin, is depicted in the last color illustration in the book. The "crowning of the American landscape" thus culminates in a wooden model hung on a wall.

ROBERT FISHMAN
Rutgers University, Camden

BRUCE ELLIS. *Bishop Lamy's Santa Fe Cathedral*. Albuquerque: University of New Mexico Press, for the Historical Society of New Mexico. 1985. Pp. xiv, 208. Cloth $19.95, paper $10.95.

Who put the merlons on the archbishop's church? Why are the nave walls of the subsequent masonry cathedral out of rectangular harmony with the façade? For years the late Bruce Ellis, a historical archaeologist with the Museum of New Mexico, pondered these and dozens of other questions about one of America's most unusual religious structures. In this candid little book he gives the answers. Taken together, they explain how a hulking, eighteenth-century, adobe parish church was subsumed, section by section, into the sturdy Romanesque vision of Jean Baptiste Lamy.

The Americanization of Santa Fe in the second half of the nineteenth century proceeded fitfully along a variety of fronts: political, economic, social, religious, and, of course, architectural. The "old adobe" style especially offended progressive-minded citizens only recently arrived from "the States." On a visit in 1880, General William T. Sherman expressed to the residents of Santa Fe the "hope that ten years hence there won't be an adobe house in the Territory. I want to see you learn to make them of brick, with slanting roofs. Yankees don't like flat roofs, or roofs of dirt" (p. 6). One Yankee, Charles F. Lummis, did, and before the decade was out his celebration of New Mexico's

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"sun, silence, and adobe" was attracting other visitors. The resulting tension is still with us.

This is not a social history of the "city different," despite the claim of the Historical Society of New Mexico. It is just what Ellis intended it to be, a historic structures report based on ecclesiastical and lay descriptions, newspaper accounts, archaeological data, and a variety of graphic materials, including insurance maps. It remains to compare the example of Santa Fe with what happened in San Antonio, Tucson, and Los Angeles and to fit this well-defined piece into the larger picture.

Who put the merlons on the archbishop's church? Thanks to Ellis's characteristically thorough sleuthing, it can now be told. Lamy did.

JOHN L. KESSELL
University of New Mexico

JAMES J. FARRELL. *Inventing the American Way of Death, 1830–1920*. Philadelphia: Temple University Press. 1980. Pp. viii, 287.

I strongly recommend this book, the first coherent narrative of the origins of present-day American attitudes and practices about death. These facets of our culture were satirized in Jessica Mitford's *The American Way of Death* (1963). But, according to James J. Farrell, such criticisms "misunderstand the historical roots . . . and thus the intellectual, institutional, and emotional roots" that underlie contemporary usages (p. 4).

Beginning with the nation's religious and ideological heritage, Farrell sketches an ideal-type of Puritanism, followed by the Enlightenment and Unitarianism, on the one hand, and romanticism and evangelicalism, on the other. This survey provides essential background and introduces some of the major themes. The second chapter examines scientific naturalism, which fostered meliorist activity, the concept of natural death, and a "species perspective." Chapter 3 studies religious liberalism and its view of death as a welcome transition to a better life. Thus, the "flower funeral" of Henry Ward Beecher in 1887 symbolized love and faith vanquishing fear and grief.

Farrell moves next into social history. The evolution of the modern cemetery is reviewed in the fourth chapter, from the initial rural-garden type in 1831 through the lawn-park model after 1855. Especially evocative is the discussion correlating the planned order of the lawn-park cemetery with Progressivism, landscape architecture, and the cult of efficiency. Chapter 5 is an informative inquiry into the "modernization" of the funeral service: undertakers became funeral directors and popularized embalming, which was considered a strange custom of ancient Egypt until about 1880. Of particular value is Farrell's analysis of the struggle between those who advocated embalming and those who advocated cremation. In the final chapter the author gives a more or less quantitative account of mortuary change in Vermillion County, Illinois.

The scope of the work is not defined clearly enough. Farrell considers the American way of death to be virtually equivalent to "the dying of death," the title of an anonymous English article (1899) affirming the "disappearance of the thought of death as an influence bearing upon practical life" (p. 4). Early in the book, when he deals with deathbed scenes, remembrance, efforts to increase longevity, and life insurance, the author signals a broad interpretation of his subject. In later chapters, however, attention is focused more strictly on cemeteries and funerals, and Farrell incorrectly assumes that "established forms of the American way of death" have not varied since 1920 (p. 15). Certainly, ideas of mortality have continued to be modified in recent decades by the biomedical revolution and an aging population.

The book's theoretical structure could have been stronger. Farrell emphasizes "middle-class opinion" as the dynamic causative force and neglects the growing power of corporate capitalism and the bureaucratic nation-state after 1865. And, as my own studies indicate, many cultural phenomena regarding mortality were furthered by positivist social thought. Farrell's work would also have benefited from acquaintance with sociological inquiries by W. Lloyd Warner (*The Living and the Dead* [1959]) and by Talcott Parsons, Renée C. Fox, and Victor M. Lidz (in *Death in American Experience*, edited by Arien Mack [1973]). Finally, Farrell's writing style is marred by repetition, excessive use of quotations, and awkward attempts at humor. His documentation is full but too often careless.

Despite its shortcomings, the book successfully follows through on the collections edited by David E. Stannard (*Death in America* [1975]) and Charles O. Jackson (*Passing* [1977]). Farrell has made a significant contribution to American cultural history.

GERALD J. GRUMAN
State University of New York, Stony Brook

E. G. ALDERFER. *The Ephrata Commune: An Early American Counterculture*. Pittsburgh: University of Pittsburgh Press. 1985. Pp. xiii, 273. Cloth $21.95, paper $8.95.

Ephrata Cloister, located a few miles from Lancaster, Pennsylvania, still stands as a monument to an eighteenth-century religious movement. The Pennsylvania Historical and Museum Commission has preserved and restored the complex of timber struc-

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tures where, from 1735, white-robed monks and nuns and married householders followed rules laid down by the mystic John Conrad Beissel. E. G. Alderfer has given us the first adequate interpretation of Beissel and of the movement he founded.

Beissel was one of the many radical Pietists who sought an ideal community in the Pennsylvania wilds. He drifted from one such group to another in Europe and, in his early days in America, associated with Dunkers (Church of the Brethren), Labadists, and the Hermits of Wissahickon. His Ephrata followers shared the Pietist enthusiasm for restoring primitive Christianity on the basis of the New Testament. This was part of Ephrata's appeal to Lutheran, Reformed, and especially Mennonite neighbors. Alderfer writes that "the central element in both the social and spiritual idealism of Ephrata was the vision of the spiritual power and cohesion of the first Christians as recorded in the Acts of the Apostles" (p. 6).

Gottfried Arnold, a Lutheran Pietist, popularized this vision in books that directly influenced Beissel. Arnold's version of primitive Christianity put great stress on abstinence from sexual activity. As Donald Durnbaugh noted in *The European Origins of the Brethren* (1958), Arnold inspired the early Dunkers to separate married couples. Although Beissel owed much to Arnold for the Ephrata pattern, Alderfer acknowledges Arnold only as a source for austere garb of community members. Beissel, however, was not simply a biblical restorationist but drew as well on many strains of mysticism and made Ephrata a haven for gnostic ideas and terminology.

Alderfer rightly emphasizes that Ephrata was a counterculture. Beissel saw it as an outpost of the coming age. His followers rejected marriage, personal property, and church and state as the props of an already dying world. When the Eckerling brothers tried to make Ephrata a profitable agricultural collective, Beissel voided their contracts and destroyed orchards and shops and returned the commune to poverty and bare subsistence. This was deliberate choice, as Alderfer indicates. Contemporary Moravians, who were more orthodox Pietists, integrated their Bethlehem and Salem communities into the local economy to support their missionary work and accepted society's other institutions.

The Ephrata counterculture attracted such men as Conrad Weiser and Christopher Sauer and, significantly, their wives. It offered women an alternative to child-bearing and drudgery. The commune developed schools, became a center for book publishing, and created a prodigious output of music and poetry.

RICHARD K. MACMASTER
Bluffton College

MARTHA CONDRAY SEARCY. *The Georgia-Florida Contest in the American Revolution, 1776–1778*. University: University of Alabama Press. 1985. Pp. x, 293. $36.50.

In this relatively brief book Martha Condray Searcy undertakes an assessment of the "1775–1778 Georgia-Florida campaigns in the overall Revolutionary military struggle" (p. ix). Drawing on materials available both in manuscript and on microfilm, Searcy details the raids, counterraids, and aborted expeditions that constituted the partisan warfare along the frontier of Georgia and Florida during the first three years of the war. Because this is a blow-by-blow account, the reader is grateful for the map included at the beginning of the book.

In the preface the author suggests that her study "was undertaken because available secondary sources do not provide a sufficiently detailed account of events in this place during this time" (p. ix). Although Searcy might have provided additional details, the story has already been told in more readable fashion. Kenneth Coleman neatly summarized this front in *American Revolution in Georgia* (1958), and Leitch Wright skillfully narrated the Florida side of the story in *Florida in the American Revolution* (1975). The native American subtheme was adequately portrayed in James O'Donnell's *Southern Indians in the American Revolution* (1973). The particularly fascinating story of partisan leader Thomas Brown was treated exhaustively by Gary Olson in articles published in the *South Carolina Historical Magazine* and the *Georgia Historical Quarterly*. (Searcy omitted the *Georgia Historical Quarterly* articles from both citations and bibliography.) Likewise, recent work on Alexander McGillivray's early life was ignored in favor of John Caughey's dated introduction to *McGillivray of the Creeks* (1938).

This assertion of originality and the puzzling disregard of secondary works are the most troubling aspects of Searcy's book. From the outset, general ideas are footnoted with references to original sources as if the author were the first ever to discuss the material. With respect to a general introduction to southeastern native American cultures, for example, the author makes no mention of Charles Hudson's fine ethnohistorical treatment but rather cites the outdated work by R. S. Cotterill. Perhaps even more amazing is her omission of Paul Smith's classic study, *Loyalists and Redcoats* (1964), which long ago explained the assumptions of the British government and the assertions of the southern loyalists that the majority of southern colonials needed only a show of British force to rally them to the king's standard.

Well-written military history can attract anyone with an interest in history; poorly written military history will tax even the most patient buff. Unfor-

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tunately, Searcy does not redeem her book with her writing style. Choppy sentences, statistics stapled together with semicolons, and pasted paragraphs remind the reader of notes taken from an orderly book. One can only wonder at such sentences as "Both armies ate whole herds of livestock" (p. 181).

Ultimately, this must be judged an unsatisfactory book. It is unfortunate that a reader did not catch the problems in the manuscript at the editorial stage so that the author and potential reviewers would have been spared embarrassment. No reviewer takes pleasure in writing a negative review. It is to be hoped that the industrious Searcy will be induced to seek better editorial help in her next venture.

JAMES H. O'DONNELL III
Marietta College

LEONARD W. LEVY. *Emergence of a Free Press*. Rev. ed. New York: Oxford University Press. 1985. Pp. xxii, 383. $29.95.

Emergence of a Free Press is a revision of Leonard W. Levy's *Legacy of Suppression*. The earlier book, controversial when published in 1960, has long since entered the realm of conventional knowledge with its thesis that the framers of the Constitution "did not believe in a broad scope for freedom of expression, particularly in the realm of politics" (p. ix). The new edition includes a great deal of new material, in accordance with Levy's revision of that thesis.

The new book calls attention to the wide difference, particularly after independence, between law and theory and to the even wider gap between law and practice. For example, Levy now regards the Sedition Act of 1798 as "a truly libertarian achievement" in its allowance of truth as a defense to libel and its requirement of proof of malice. He provides proof of his new ideas in copious references to the wide extralegal freedom enjoyed by the press even before 1798. This is persuasively done and constitutes a major new contribution.

Levy professes to be "puzzled by the paradox . . . of nearly unfettered press practices in a system characterized by legal fetters and the absence of a theory of political expression that justified those press practices" (p. xvii). He need not be. The gap between theory and practice is a staple in studies of political systems, and lawyers frequently call attention to a similar gap between law and practice. We may, indeed, posit a conclusion that theory follows practice as often as not. Contrary to Levy, I would argue that freedom is more a result of attitudes and politics than of theory or law. Old-style sedition laws such as those of 1798 or 1918, are literally unenforceable just as are laws requiring merchants to have watering troughs in front of their stores.

That new sedition laws can be adopted and applied remains, of course, a danger. What we can say is that they will not define sedition in the same way as it would have been conceived during the prerevolutionary era. We now believe in, and have institutionalized, the concept of a loyal opposition. Mere opposition to the government of the day can no longer be regarded as seditious, even when it is rambunctious, fractious, and careless of the truth. The danger is to groups or opinions far beyond the democratic consensus, but even this danger is now largely confined to periods of emergency. No one should be surprised that patriots used the concept of sedition to suppress Tories, for every crisis in the national existence produces similar reactions. Nevertheless, we have come a long way since 1798, so far that I cannot be greatly concerned that the Sedition Acts of 1918 and 1940 are still on the books. I am happy that they are no longer enforced, and I assume that only a great national crisis will bring them back to life.

LOREN P. BETH
University of Georgia

DWIGHT F. HENDERSON. *Congress, Courts, and Criminals: The Development of Federal Criminal Law, 1801–1829*. (Contributions in American History, number 113.) Westport, Conn.: Greenwood. 1985. Pp. x, 257. $35.00.

Legal historians have paid relatively little attention to the growth of federal criminal justice in the United States. Dwight F. Henderson's informative monograph, which builds on his previously published *Courts for a New Nation* (1971), provides the first comprehensive account of federal criminal legislation and its enforcement during the era of Jeffersonian Republicanism.

The Republicans inherited a rudimentary and politicized system of criminal justice from their Federalist predecessors. The Judiciary Act of 1789, which established the lower federal courts, did not permit appeals to the Supreme Court in criminal cases. Congress further weakened the new structure by empowering state judges and officials to enforce federal laws and by requiring the use of state rules of procedure in federal district and circuit courts. With the rise of party strife in the 1790s, federal marshals, attorneys, and judges increasingly owed their appointments to political patronage. The embattled Federalists also came to rely on criminal laws, such as the notorious Sedition Act of 1798, to suppress criticism of their policies.

Jefferson and his successors took some modest steps to promote uniformity in criminal jurisprudence. The Judiciary Act of 1802 permitted circuit judges to certify disputed points of criminal law to

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the Supreme Court for a final decision. From 1802 to 1829 the Marshall court ruled on twenty-one such referrals and thus created a small body of authoritative federal precedents. Since the court still lacked general appellate jurisdiction over criminal cases, however, it could neither control the workings of inferior tribunals nor ensure that a defendant's procedural rights would be consistently protected.

The narrow range of offenses that were cognizable by federal judges proved a further obstacle to the development of a strong national system of criminal justice. The Marshall court ruled in 1812 that only statutory crimes could be prosecuted in federal courts. Although Congress enlarged the number of such specific offenses in the Crimes Act of 1825, it refused to grant the federal judiciary general jurisdiction over common-law crimes, such as that enjoyed by state judges.

Congressional legislation defined three types of crime: familiar offenses, such as murder or forgery; acts involving a federal agency, such as the post office and the Bank of the United States; and behavioral controls that addressed unique problems or temporary emergencies. The first two categories gave law enforcement officials little trouble; the third, which dealt with ideological and moral issues, provoked widespread popular resistance and evasion. Henderson devotes six of his nine chapters to a detailed analysis of these controversial measures, which included neutrality laws, coercive economic regulations designed to implement Jefferson's embargo and Madison's nonintercourse policies, the control of "alien enemies" in the War of 1812, and efforts to suppress piracy and the international slave trade. The harsh punishments attached to many offenses frustrated prosecution efforts, as did a lack of coordination between customs officials in the Treasury Department and marshals and district attorneys. Juries were an essential safety valve in what remained a highly politicized system of criminal justice.

Based on extensive research in manuscript court records, Henderson's lucid study fills an important gap in the legal history of the early republic.

MAXWELL BLOOMFIELD
Catholic University of America

WILLIAM H. MASTERSON. *Tories and Democrats: British Diplomats in Pre-Jacksonian America*. Foreword by FRANK E. VANDIVER. College Station: Texas A & M University Press. 1985. Pp. xvi, 280.

This book by the late William H. Masterson consists of short biographical sketches of the first nine British envoys to the United States from 1791 to 1823: George Hammond, Robert Liston, Edward Thornton, Anthony Merry, David Erskine, Francis ("Copenhagen") Jackson, Augustus Foster, Charles Bagot, and Stratford Canning. A separate chapter is devoted to each. All these envoys, when in the United States, held a low rank, and Thornton was only a chargé. Another chargé, John Morier, who served after Jackson's dismissal for about a year, is considered briefly. The book is thoroughly researched and has ample footnotes and a long bibliography. Pictures are included of all the envoys except Jackson and the two chargés, and also of three wives of the diplomats. The author writes that his objectives were to present these envoys in greater perspective than had previously been done and to describe their unique perceptions of pre-Jacksonian America. He does give more thorough accounts of the envoys than can be found elsewhere. All of them except Erskine were Tories, and only Erskine, the Whig, "dimly perceived during his American mission any real, deep current of American nationality" (p. 223). Relying almost exclusively on Federalist contacts, the envoys consistently misreported the American scene to their superiors in London. On the whole, they were narrow, mediocre men (with the exception of Canning and perhaps of Liston and Hammond, who were, however, poor envoys to America), but one wonders whether such frequent characterizations as dazzled, spellbound, fatuous, and confused are not overdone.

The book's only disappointment relates to Masterson's second objective. The envoys' unique perceptions are seldom disclosed. Thus, Jackson wrote "a mammoth, comprehensive view of the society, economic prospects, and political structure of the United States" (p. 142), and Foster compiled *American Notes* (on the years 1805–07 and 1811–12). Brief indications of the contents of these and of other similar writings by other envoys would be interesting. Indeed, the promise of the title, *Tories and Democrats*, is not lived up to. The reactions of these upper-class Britons, however imperceptive most of them were, could provide insights into the merits of aristocracy and democracy. As it is, we learn little more than that these supercilious men, heartily backed by their wives, detested America and found Washington an abomination.

Since all but two of these men served before 1812, the book is in effect a study of the coming of hostilities. Perhaps its greatest contribution is to make evident, by implication, how important ideology was in provoking the War of 1812. Not only the envoys detested America; the British ruling classes did too. They believed, correctly, that Americans returned the dislike in full measure, and they viewed the Republicans, especially, as dangerously radical and incorrigibly pro-French. Amid such strong dislikes, diplomacy had little or no chance of succeeding. Stressing ideology, the book usefully

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supplements other accounts of the background of the war.

In short, even if not profound, this is a valuable book, beautifully and amusingly written, and one is grateful that Masterson completed it before his tragic death. Unlike much historical writing, this book is a pleasure to read.

CHARLES S. CAMPBELL
Claremont McKenna College

MARY W. M. HARGREAVES. *The Presidency of John Quincy Adams*. (American Presidency Series.) Lawrence: University Press of Kansas. 1985. Pp. xv, 398. $25.00.

This latest addition to the American Presidency Series, published by the University Press of Kansas, could not be more welcome. When so much is being published about Andrew Jackson and his times, one tends to forget or skip over the presidential years of John Quincy Adams, except to comment on his political ineptitude, the "corrupt bargain," and possibly his great state paper to Congress in 1825. That "crazy old man," as Jackson later called him, deserves better, and he has received it in this thoughtful and valuable study by Mary W. M. Hargreaves.

What makes this book unique is its determination to do more than narrate the events of the Adams administration. Rather, Hargreaves offers a compelling thesis and does an able, if not totally convincing, job of demonstrating its validity. Simply stated, the thesis contends that Adams and his associates, by their nationalistic program, sought to "harmonize divergent sectional interests" (p. xiii). This program, which brought Adams and Clay together in the first place, accounts for their efforts to broaden U.S. cooperation with Latin America and for their concern in domestic affairs for the integrated economic development of the nation as a whole. In this account Richard Rush, as secretary of the treasury, emerges as an extremely important member of the administration in advancing Adams's nationalistic designs—much more so than has been recognized heretofore. Samuel Southard (Department of the Navy) and James Barbour (Department of War) also assume more important roles in the articulation and development of the presidential program. The failures and frustrations suffered by the Adams administration are attributed to the political obstructionism of the opposition, which pursued sectional, local, and even personal advantages with no common purpose except that of electoral success.

The weakest sections of this book treat some key issues of the administration such as the "corrupt bargain." The author appears to want it both ways: at one point she fairly exonerates Clay and Adams of wrongdoing; at another she suggests possible culpability. Her discussion of the House election in 1825 is not as helpful as it might have been. For example, she neglects to discuss Missouri's vote, although she later contends that it was "critical" (p. 198). If "corruption" on the part of Adams can be documented, it surely must center on what he promised the representative from Missouri. As for the Kentucky vote in the House election, the author states that Kentuckians were not predisposed toward Jackson's candidacy because of his criticism of their militia during the Battle of New Orleans; therefore, the instructions sent by the Kentucky legislature to its delegation in Congress "connoted little more than a preference for a regional candidate" (p. 251). But, after Jackson, who were the other regional candidates among the three men?

Although Hargreaves provides considerable evidence that the lack of adequate organization contributed substantially to Adams's defeat for reelection in 1828, she argues that sectionalism constituted the principal cause, which naturally accords with her thesis. All of her descriptions and analyses of the characteristics and personalities of the leading figures of the period are right on the mark. These pages constitute some of the best in the book. Unfortunately, she chose not to comment on Adams's remark, explaining his handling of domestic policy, about being "palsied" by one's constituents. Nevertheless, this is far and away the best account we have of the unlucky but honorable administration of John Quincy Adams.

ROBERT REMINI
University of Illinois, Chicago

JOHN M. BELOHLAVEK. *"Let the Eagle Soar!": The Foreign Policy of Andrew Jackson*. Lincoln: University of Nebraska Press. 1985. Pp. x, 328.

This is the first comprehensive study of Andrew Jackson's foreign policy. Its interpretation differs from traditional accounts in that the author, John M. Belohlavek, argues that Jackson took charge of foreign policy and shrewdly mixed bluster and anger with occasional displays of reason both to confuse the nation's rivals and to convince them that it was wise to settle issues peacefully. Furthermore, the president "eagerly pursued a policy of promoting commercial expansion, demanding worldwide respect for the American flag, restoring American prestige and national honor, and fostering territorial growth" (p. 2). Jackson was a proponent of manifest destiny before the label appeared, for he firmly believed that America's welfare lay in expanding control to the Pacific. Through diplomatic representatives, treaties, and the American navy, he sought commercial ties with Latin America, the

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Middle East, and the Far East. During his presidency the American eagle "began to soar" (p. 3).

Belohlavek declares that Jackson's diplomacy was largely a success. The president temporarily shelved his Anglophobia in resolving the West Indian trade issue and in attempting to settle the longstanding Canadian boundary dispute. With Spain he achieved lowered tonnage duties and a commercial treaty, and with several countries, including Spain and Portugal, he resolved claims issues. Jackson also achieved a "qualified success" (an overused term in this study) in establishing trade with Russia, North Africa, the Mediterranean states, and South America. While effectively chastising the pirates of Sumatra, Jackson used persuasion in the Far East by sending Edmund Roberts to spread American commercial interests. In Latin America Jackson sought a favorable settlement of the Mexican border dispute, the acquisition of Texas, commercial treaties with the Central American republics and Mexico, and the possible construction of a canal through the Isthmus of Panama. The spoliation claims issue with France posed the most serious problem in foreign affairs for the Jackson administration. After considerable controversy, Jackson (with strategic displays of anger and with the help of Edward Livingston, John Forsyth, and Martin Van Buren) reached a compromise with the French in late 1835 that maintained America's honor and won Europe's respect.

This will be a handy book for historians, but, except for certain details (sometimes too many), much of the material is familiar and rests almost exclusively on American sources. Belohlavek concludes that Jackson followed the "most expansive and aggressive foreign policy" between the administrations of Thomas Jefferson and James K. Polk and that he laid the basis for "the more dynamic diplomatic actions of the modern presidency" (p. 257). These are overstatements. Most of the administration's achievements were modest and helped build a base for future successes—which is not the same as asserting that Jackson brought about the "revitalization of the American commercial empire" (p. 89). Despite the administration's accomplishments, the northeast boundary remained unsettled, the nation's problems with Latin America worsened (partly because of Jackson's own appointments to diplomatic posts), and the Spanish refused to grant substantial concessions in Cuba and Puerto Rico. It is also difficult to find anything "dynamic" or "diplomatic" about Jackson's dealings with Texas and Mexico. And one even wonders whether a greater emphasis on negotiation and understanding might have eventually resolved the French crisis without the threat of war. Belohlavek brings together much disparate information and draws more attention to foreign affairs during these tumultuous years. But the need still exists for a study based on multiarchival research.

HOWARD JONES
University of Alabama

JOHN H. SCHROEDER. *Shaping a Maritime Empire: The Commercial and Diplomatic Role of the American Navy, 1829–1861*. (Contributions in Military Studies, number 48.) Westport, Conn.: Greenwood. 1985. Pp. 229. $35.00.

John H. Schroeder has undertaken the rather ambitious task of writing a coherent account of the expansion of the nation's "commercial diplomacy" during the three decades preceding the Civil War, with emphasis on the navy's role therein. As he points out, "Given the uncoordinated nature of American commercial activity abroad, the navy's antebellum development occurred in a rather haphazard and unsystematic fashion" (p. 7). This fact and his effort to describe the activities of commercial visionaries and naval reformers, regardless of success, result in a certain lack of coherence.

The author holds that this period witnessed a dramatic increase in the navy's peacetime role. He seems to demonstrate, however, that the navy's role remained largely unchanged from that of the period immediately after 1815; it was simply extended into other geographic areas—most notably, the waters of Latin America and the Far East. Several exploring expeditions did sail after 1829, but the first had been proposed by President J. Q. Adams in 1825, and only two could be considered major undertakings. The navy established four of the distant cruising stations before 1829; of the two added thereafter, the Africa Station's warships were not primarily intended to support "commercial diplomacy." This hardly sustains the assertion of a dramatically increased naval role.

Although the bibliography includes a number of primary sources, little in this book cannot be found in the impressive list of secondary works, from which Gerald S. Graham's *The China Station: War and Diplomacy, 1830–1860* (1978) is surprisingly omitted. More careful use of these works would have prevented a number of errors. For example, the U.S. warships that returned to the Orient in 1865 were not "part of an entirely different unit, the Asiatic Squadron" (p. 185); they continued to form the East India Squadron until their station's name, and little else, was changed some months later. Schroeder does not mention Captain John Percival's voyage of circumnavigation in the *Constitution* 1844–46, which was ostensibly intended to increase American trade with the east coast of Africa and the East Indies, and virtually ignores the importance of

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foreign naval forces, especially those of Britain, in fostering American commercial expansion.

Finally, Schroeder seems not to understand the navy itself. One example must suffice: he asserts that the destruction of Quallah-Battoo in 1832 "seriously affected [Commodore John] Downes' naval career. He never again commanded an American warship, and he spent the remainder of his career performing shore duties" (p. 28). Nonsense! Many senior officers preferred shore duty to the discomfort of life afloat. Downes's subsequent career reveals no indication of official displeasure, and, as Schroeder notes, the commodore declined command of the East India Squadron in 1841 (p. 50).

ROBERT ERWIN JOHN
University of Alabama

BRECK ENGLAND. *The Life and Thought of Orson Pratt.* Foreword by LEONARD J. ARRINGTON. Salt Lake City: University of Utah Press. 1985. Pp. xvi, 359. $14.95.

Breck England's book traces the career of nineteenth-century Mormonism's foremost intellectual. Orson Pratt was born in 1811 in upstate New York, became a Mormon at nineteeen and an apostle at twenty-three, and was an unwavering advocate and missionary of Mormonism throughout his long life. He was "unprespossessing" in appearance, often lost in thought, ten times married, and a financial failure (p. 201). Although unschooled, he was committed to inquiry and logic and sought constantly to understand the workings of the divine by integrating the teachings of Joseph Smith with nineteenth-century theology and science. What he conceived clearly he expressed lucidly from the pulpit and in the "mountain of tracts" and books that issued from his pen (p. 202). Yet his "pursuit of the profound" often led him into difficult and obtuse positions (p. 298). He lacked Brigham Young's ego and bombastic courage yet met philosophy and science on their own ground. Even before Joseph Smith's death in 1844, Pratt had become a major public voice of Mormonism. Later he was so often spokesman for the church that, although Americans "saw Brigham Young's rigorous countenance" when they visualized Mormondom, "the voice" they "heard was Orson Pratt's—placid, stoical, but uncompromising in its own logic" (p. 238).

In significant ways Pratt was a counterpoint to Brigham Young. When Young led people out of the world, Pratt's intellectual path tended toward the world, seeking issues common to the Mormon restoration and the realms of theology, philosophy, science, and logic through which the Mormon message could convince the world. As they entered Salt Lake Valley, however, Young's eyes were fixed on practical considerations whereas Pratt saw formulas and equations as well as biblical symbol and prophetic metaphor and tied the "Mormon exodus" to ancient Israel in his teachings.

Pratt often crossed swords with Young. No match for the dogmatic church president, he was repeatedly beaten down. Young forced him to make public confessions of heresy, suppressed his books, sent him on disciplinary missions, and downgraded him in the church hierarchy. Neither these humiliations nor his bookish absent-mindedness, however, reduced Pratt's popularity with rank-and-file Mormons who flocked to his sermons and lectures by the thousands. To them he was a legendary figure comparable in some respects to J. Golden Kimball, whose independence at the pulpit delighted early twentieth-century Mormons.

Good insights abound in this book, yet much in it is difficult to understand. It suffers from the general problem of Mormon communication noted by Sidney Ahlstrom and is complicated by Pratt's own thought. Although England commands his material thoroughly and writes many vivid passages, one wishes for a stronger interpretive hand and more willingness to readdress traditional Mormon assumptions. On the whole, however, this is an important and successful book that compares favorably with other good works recently published on Mormon history.

CHARLES S. PETERSON
Utah State University

RICHARD S. VAN WAGONER. *Mormon Polygamy: A History.* Salt Lake City, Utah: Signature. 1986. Pp. vi, 307. $19.95.

In the political rhetoric of the morally smug, slavery and polygamy in mid-nineteenth-century America constituted the "twin pillars of barbarism." The opponents of both institutions used them to sum up all that they despised in two cultures, southern in one case, Mormon in the other. That was perhaps odd, since neither slavery nor polygamy was a universal practice in the cultures they came to symbolize. Only 20 to 40 percent of Mormon families were polygamous. Of the polygamous males over 66 percent had only two wives, and fewer than 5 percent married five or more women. These statistics suggest strikingly analogous observations about the slaveholding patterns of the antebellum South. Nonetheless symbols carry weight, and, quite apart from the overstatements of critics, southerners and Mormons invested considerable energy into making their peculiar institutions stand for the virtue of their respective cultures. Slavery and polygamy acquired a sacred status and became a badge of group identification that sent their defenders into war against the authorities of the United States.

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Scholars now make distinctions between Mormon polygamy and southern slavery, for plural marriage in the Utah territory was neither a forced option for women (at least no more than other options) nor a status without legal escape. Richard S. Van Wagoner, in a clear and well-researched book, has provided a narrative account of Mormon polygamy that is useful to a general and to a specialized audience. Van Wagoner, doubtlessly with a certain pride, claims descent from Mormon polygamists, but he has kept a sufficiently critical eye turned on what his ancestors did. Although he has been through the documents in the Mormon archives, he wisely has not tried to settle the many contentious issues about origins. Van Wagoner has focused on responses and consequences. Therefore, readers will not come away from the book absolutely certain about when Joseph Smith hit on the idea of plural wives or when he began to act on it. They will, however, learn a great deal about the unenthusiastic reaction of Smith's first wife to her husband's inspiration as well as the storm of opposition that public acknowledgment of the practice aroused among church members. Van Wagoner is frank about other things as well: the duplicity of church leaders in interpreting and enforcing their decrees against polygamy, the unhappiness of many Mormon women in living out the role of an "extra," and the violence that has attended the actions of Mormon fundamentalists who have continued to practice polygamy into the present.

Van Wagoner's book stays mainly within conventional channels. In only one chapter does he venture the kind of inquiry that will excite social historians or, for that matter, feminists. The reader sees little of the dynamics of plural marriage, the interaction of husband, wives, and children. That is too bad because Mormon polygamy is one of the few sustained examples of alternate family arrangements that American history can furnish. It may have been oppressive, as sponsors of the "twin pillars" theory wanted to assert, or it may have provided liberating opportunities, as some feminists have recently wondered. These issues remain largely unexplored, and one hopes that Van Wagoner's book will free future analysis from some of the tired questions that have encrusted the historiography of Mormons. We might then, for starters, ask why former Mormon polygamists lined up with Republicans, the public moralists who had linked them to southern slaveholders, rather than with Democrats, whose cultural styles tolerated deviance from Protestant evangelical norms.

R. LAURENCE MOORE
Cornell University

CLYDE A. MILNER II and FLOYD A. O'NEIL, editors. *Churchmen and the Western Indians 1820–1920.* Norman: University of Oklahoma Press. 1985. Pp. xvi, 264. $19.95.

Interest in the role of missionaries as cultural agents seems to be growing, as is reflected in this collection of articles surveying the lives and careers of six of these agents among separate American Indian groups. The intention of the editors, Clyde A. Milner II and Floyd A. O'Neil, is to present a multidimensional analysis of different denominations interacting with different native societies that is aimed at providing a model for future comparative studies. Although the papers present some useful information and some interesting comparative insights, the collection falls short of its goal.

The tasks faced by both editors and authors were difficult. For their part, the editors were confronted with six very different papers, each of which dealt with a different denomination, a different Indian group, and a different time period. They sought to solve this problem by dividing the book into three thematic sections. The first contains sketches of Presbyterian Cyrus Byington and Methodist John Jasper Methvin by W. David Baird and Bruce David Forbes respectively and focuses on the "Protestant paradigm." The second, consisting of O'Neil's sketch of Mormon George Washington Bean and Robert C. Carriker's of Jesuit Joseph M. Cataldo, centers on the issue of frontier variations. Finally, Clyde A. Milner II's sketch of Quaker Albert K. Smiley and Martin Zanger's of Episcopal bishop Henry B. Whipple are discussed in the context of national reform figures. This organizational structure is an excellent one and provides ample opportunity for the sort of comparative analysis that the editors had in mind.

The authors were faced with the much more difficult task of surveying the lives of these individuals in a manner calculated to realize the comparative potential provided by the editors. Here the book falls short.

In the first section the reader is treated to excellent sketches of two nineteenth-century Protestants who, by very different processes, ended up laboring among Indians in Oklahoma: Byington among the Choctaws at mid-century and Methvin among the Kiowas at century's end. In the course of these discussions, we learn of Byington's identification with the politically ambitious mixed-blood population, but we also learn of his linguistic skills and of his insistence on reaching the more conservative full-blood Choctaws by preaching and conducting instruction in their native language. We learn, too, of Methvin's more restrictive approach, which insisted on English-language instruction, hair cutting, and other symbolic representations of "civilization." What we do not learn is the degree to which these differences in approach were the result of differ-

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ences in chronology, in denominational affilations, in the Choctaw and Kiowa cultures, or in the individual temperaments of the missionaries involved. Parts 2 and 3 are beset with similar problems. The treatments of Bean and Cataldo are excellent, but beyond learning that the situations of the Mormon and Jesuit missions were not typical in the western United States, little comparison can be drawn. The discussions of Whipple and Smiley afford an excellent survey of assimilationist thought and behavior in post–Civil War Indian affairs but little insight into the comparison of specifically Quaker and Episcopal views. Moreover, we are either told outright or suspect that these individuals are not especially typical of either missionaries or lay people within their own denominations or in the population at large. Only Bean seems typical of his community, and, as we are informed, his community was notoriously atypical. Hence, although these individuals may have been agents, we learn little about what or whom each represented.

A more severe problem than this, however, is the lack of insight (comparative or otherwise) into the religious character of the Indian cultures in the relationships discussed. In all six cases, Indian groups were in the throes of acculturative crises, leading them to accept (and in some cases to invite) the intrusion of Christian missionaries. We are told only of the materialistic motives that promoted conversion, as in the case of the Kiowa convert who is quoted as saying that he liked Christianity because his conversion had earned him "fifty acres of land in cultivation, a good crop, [and] some cattle" (p. 62). No doubt, material acquisition played a role in these peoples' desire to learn of the white man's God, but to make this their sole motivation and to ignore the forms and processes of spiritual accommodation is to miss half the point and most of the drama in these situations. Indeed, as the editors point out, "partial acceptance of Christianity may reveal an adaptive form of cultural self-determination" (p. xii), but we see nothing of the internal workings of this adaptation.

Even though each chapter is an excellent biographical sketch of which each author may be justly proud, this lack of attention to Indian spiritual dynamics and the problem of atypicality inherent in isolated biography cause this book to fall short of its goal of providing a model for multidimensional studies of cross-denominational intercultural relations.

The editors do deserve congratulations for their organizational skills and for their stunning vision. In fact, despite their failure to realize it in this book, the editors' demand for "a broader understanding of the complexities of religion and culture as they affect societies and individuals" (p. xvi) represents a contribution to the field of intercultural studies that should inspire further work in this field.

CHRISTOPHER L. MILLER
Pan American University

BRIGHAM D. MADSEN. *The Shoshoni Frontier and the Bear River Massacre.* (Utah Centennial, number 1.) Foreword by CHARLES S. PETERSON. Salt Lake City: University of Utah Press. 1985. Pp. xxi, 285. $19.95.

The purpose of this book is to illuminate one of the "longest and least known" (p. x) periods of conflict between Euro-American settlers and native inhabitants in U.S. history: the Shoshoni Indian wars of the 1840s, 1850s, and 1860s. These wars—really an extended series of skirmishes and retaliatory raids—culminated in the Bear River massacre of January 1863.

Brigham D. Madsen is a knowledgeable author. Having written or edited ten other books on the Bannocks and Shoshonis and taught in Utah for many years, he now turns his attention to a subject everyone has overlooked. The result is an informative chronicle of antagonism between the far-flung, loosely affiliated Shoshonis and a cavalcade of western travelers, Mormon settlers, and jumpy soldiers.

Madsen begins by sketching the Indian groups that inhabited the northern Great Basin in the 1840s on the eve of the white onslaught. He describes how Euro-Americans disrupted and destroyed the delicate subsistence patterns established by the Shoshonis. Cattle and horses ate and trampled the native grasses that provided seeds for food and material for baskets and shelter. The new settlers offered nothing in return but infectious diseases and liquor.

The isolation and barrenness of the Shoshoni homeland multiplied the impact of non-Indian settlement. Arbitrary lines separated the Oregon from the Utah superintendencies, requiring some Shoshoni bands to deal with officials on the Pacific coast and others to seek relief in Salt Lake City. Agents were slow to identify—let alone assist—the many Shoshoni groups, and settlers were too willing to attack one band for the sins of others. These problems produced a decade or more of violence in the Great Basin and led almost inevitably to the Bear River massacre, in which Colonel Patrick E. Connor's California volunteers killed two hundred fifty northern Shoshonis.

Despite its value as a solid study of a neglected subject, this book has shortcomings. Most striking is the author's complete reliance on conventional historical methods and the records of the National Archives. The study is not ethnohistorical; it explores Euro-American actions and leaves the Indians as shadowy figures without motives or strategies. Did the Shoshoni bands interact? Did this interac-

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tion change over time? How did conflict with whites alter their political practices? Answers to these questions do not appear here. The book's emphasis on white action may account for its brevity. Shorn of its appendixes, its often-repetitive summary statements, and its introduction, the book would hover close to the minimum size for a monograph. It would have benefited from a fuller discussion of both sides of the cultural dymanics of the Shoshoni frontier. As it is, however, Madsen's study is an excellent beginning in our effort to understand the history of Utah and the Great Basin.

FREDERICK E. HOXIE
Newberry Library

JULIA FLOYD SMITH. *Slavery and Rice Culture in Low Country Georgia, 1750–1860*. Knoxville: University of Tennessee Press. 1985. Pp. xiv, 266. $19.95.

Julia Floyd Smith argues that slavery and the rice industry in coastal Georgia differed markedly from plantation operations elsewhere in the South. Unlike cotton cultivation, rice culture was circumscribed by geography and the specialized nature of the industry. Rice lands lay in low areas, where fresh-water river tides irrigated the fields. Georgia's rice frontier thus was restricted to an area that was from ten to twenty miles wide. Rice production required managerial efficiency, a large labor force, and considerably greater outlay in equipment, machinery, and slaves per unit than did cotton plantations. Rice plantations on Georgia's coastal strip were more isolated, more specialized, and contained an average of 50.7 slaves in 1860. In contrast, cotton units in the lower Piedmont averaged 15.1 bondsmen per farm.

Rice production demanded precise workmanship at tasks better suited to an individual worker than to a gang of laborers. By rewarding slaves with free time, the task system fostered initiative among them to complete the work within a reasonable time. And, according to Smith, this system offered bondsmen "a lifestyle somewhat different and perhaps less debilitating and demeaning emotionally than [that of] slaves in other plantation belts" (p. 62). On completing their day's tasks, coastal slaves relaxed, socialized, fished, hunted, and sold their surplus wares and produce.

Acculturation of the slaves was less complete on Georgia's large estates than on smaller plantations and farms. The large units, formal and efficiently controlled, provided less contact between blacks and whites. As a result, the coastal blacks "retained their African cultural elements more completely than did slaves in other plantation settings" (p. 182). The isolation and size of the rice plantations provided the bondsmen more freedom to fashion their own Afro-American world view in religious and folk expression.

Smith concludes that slave life in low-country Georgia compared favorably with slavery elsewhere in terms of physical well-being. Slave diet, housing, clothing, and health care on the coast surpassed that provided on smaller inland plantations. Some slaves, especially drivers, house servants, and mulatto offspring, received special treatment, including modest annuities and extraterritorial manumission. Rice planters, asserts Smith, "were of that elite class of slaveholders whose esprit de corps determined the treatment of their 'people'; this treatment was generally less harsh than slave treatment in other areas of the Old South" (p. 208).

Nevertheless, Smith notes that most rice planters were efficient capitalists who viewed their bondsmen as mere entries on a ledger sheet. The steady increase in rice production, slave population, and annual returns from their investments attest to their success. Smith supports her argument for the relative mildness of coastal slavery, but the thesis needs to be tested in far greater depth. We also need to know more about the economic motivation that lay beneath the planters' alleged largesse. For the slaves' part, although some expressed affection for their masters, "the vast majority . . . longed to be free and cared little for the white overlords whose way of life caused their degradation" (p. 9). When viewed in this crucial light, then, rice slaves were not so different after all.

JOHN DAVID SMITH
North Carolina State University

JOHN CIMPRICH. *Slavery's End in Tennessee, 1861–1865*. University: University of Alabama Press. 1985. Pp. 191. $27.50.

In his solid account of race relations in war-torn Tennessee, John Cimprich argues that, although federal officials and northern reformers provided crucial assistance, the former slaves' own actions gained them new privileges despite resistance by white Tennesseans. Cimprich grants that current scholarship has proved that many slaves were assertive. The key question, he maintains, is how many exhibited this trait rather than that of personal dependency. He supports the school that contends that most slaves were independent minded, but his evidence and analysis are not entirely convincing. This is partly because of the lack of sources. To determine with any degree of certainty how many slaves asserted their independence is impossible. Moreover, extant sources are unbalanced. As Cimprich admits, "seemingly normal behavior of

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loyal slaves provoked little contemporary comment" (p. 4); most available information deals with disloyal slaves. Therefore, there is little in this work about loyal slaves or about how many blacks were victims of paternalism. Moreover, Cimprich is careless with such words as "all," "many," and "most." This only serves to reinforce the bias inherent in his sources. For example, after discussing the miserable conditions in contraband camps, he concludes that "most" blacks still favored freedom over slavery. I do not doubt this judgment, but it is supported by only two narrow sources. Elsewhere, he argues that "contrabands suspected the contract labor program might restore slavery" (p. 71). One wonders if all contrabands held this view.

Despite these limitations, this study is a valuable addition to historical literature. Cimprich's analysis of the roles whites played, how they differed in motivation and policy, and how these differences affected blacks is especially strong. We also learn about the various ways disloyal slaves exhibited their disloyalty, what they wanted for themselves and their families, and how they reacted to various federal policies and to the war.

In its broad strokes little here is new. Cimprich himself concludes that, "despite local variations, emancipation remained basically the same social phenomenon throughout the South" (p. 5). But it is the details and subtleties that should be of interest to historians. Although Leon Litwack summarized this phenomenon well, detailed state studies such as Cimprich's are necessary to refine that picture. Joel Williamson's study of South Carolina, Clarence Mohr's work on Georgia, C. Peter Ripley's monograph on Louisiana, and my recent work on North Carolina all describe similar patterns in different states and under very different circumstances; each contributes a little more to our overall understanding of how race relations evolved during and after the Civil War.

ROBERTA SUE ALEXANDER
University of Dayton

ARCHIE K. DAVIS. *Boy Colonel of the Confederacy: The Life and Times of Henry King Burgwyn, Jr.* Chapel Hill: University of North Carolina Press. 1985. Pp. xiv, 406. $29.95.

Henry King Burgwyn, Jr., belonged to a worthily prominent family, rose to the rank of colonel in the Confederate army when still only twenty years old (succeeding Zebulon B. Vance in command of the Twenty-sixth North Carolina Regiment), and was killed during the Battle of Gettysburg. Burgwyn wrote a number of historically significant and insightful letters, mostly to his mother, which have been preserved. It is mainly these letters, plus the other related primary source material that Archie K. Davis has gathered and exposited on, that render this book of particular value; it will appeal primarily to Civil War buffs and to those readers with a special interest in North Carolina history.

Obviously a labor of love, the book certainly represents a great deal of work. The quality and scope of research is quite admirable, and the writing is carefully crafted. But Davis fails to resolve the problem of the unfortunate reality that the data on young Burgwyn is insufficient to underpin a full-scale biography. The "life and times" approach here is somewhat flawed: all too often, and in passages much too lengthy, the subject is totally lost from sight.

One of the youngest field-grade officers in Confederate service, and perhaps the youngest of all the full colonels, Burgwyn apparently deserved his high rank, even at so tender an age. He possessed, and developed, a number of sterling qualities and personal traits. He was an honor graduate both of the University of North Carolina and of the Virginia Military Institute. He acquitted himself well and intelligently, interacting effectively with other people. He even could "make a good team" with superiors whom he regarded in a partially critical light. Ambitious, but not vaingloriously so, he consistently manifested a desire to lead. And he always managed to elicit unstinting devotion from his men. Only his last brash act, which brought his mortal wound, might be questioned or considered pitiable: he grabbed the regimental colors and tried to carry them in a frontal assault.

A lifelong admirer of Thomas J. Jackson, Burgwyn seems to have had equally as complex a psyche. He was genuinely religious, even though somewhat callous regarding slave breeding. Candid and pragmatic, he looked to the future, making long-range suggestions to his father. Insightful, he reflected not only on the things that happened about him but also on the larger war. Young Burgwyn's relationships with family members—most especially with his mother but also with his siblings—are extremely interesting. And apparently he had an incredibly intense love affair: the disappointed young lady never married and wore mourning for the rest of her life, another forty-seven years.

Davis handles these psychological matters reasonably well, as indeed he handles every aspect of the work with commendable quality of effort. It is a good and handsome book—the pictures are nice—but a shorter and more artfully analytic study might have been much better.

HERMAN HATTAWAY
University of Missouri, Kansas City

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BRIAN GREENBERG. *Worker and Community: Response to Industrialization in a Nineteenth-Century City, Albany, New York, 1850–1884.* (State University of New York Series in American Social History.) Albany: State University of New York Press. 1985. Pp. ix, 227. Cloth $34.50, paper $12.95.

Writing in the tradition of the new labor history, in this book Brian Greenberg nicely explicates the efforts of the Albany working class to build a labor movement between mid-century and 1884, by which time labor had won an end to prison contract labor and had turned its back on much of its more radical past. The strength of this slim book (there are less than one hundred fifty pages of text), however, lies not in its originality but in its incorporation and integration of some of the best thinking about mid-nineteenth-century working-class life. Blending Eric Foner's focus on free labor ideology with the work on artisan republicanism in the second quarter of the nineteenth century by Alan Dawley, Bruce Laurie, and Paul Faler, Greenberg establishes an interpretative framework for understanding Albany workers' responses to industrial capitalism. The organization of the Albany working class in the late 1850s, the central role of the iron molders in that process, the articulation of a "cooperative commonwealth" philosophy in the late 1860s, the defensive position of the depression years, and the role of interclass politics in shaping working-class consciousness in the 1880s, make for a compelling, well-argued story. It is also familiar, not unlike the story in neighboring Troy and Cohoes.

In addition to its synthetic strengths, Greenberg's book makes other contributions. The most interesting and original parts are short chapters on the history of the Albany Penitentiary and the fifty-year struggle against New York state prison contract labor. He also adds to our meager knowledge of the cooperative movement.

Still, there are some striking omissions. Greenberg's book offers no demographic profile of the city, and its discussion of ethnic working-class culture is skimpy and undeveloped. Moreover, the author tells us next to nothing about women, family life, or the work process, all of which we have come to recognize as essential in shaping the nineteenth-century working class. Greenberg has nicely integrated other historians' conceptual notions into his recounting of the Albany working-class experience, but the modest evidence presented does not make clear whether these ideas are sufficiently appropriate to understanding the history of Albany's working class. The author has missed some opportunities to break new ground.

DANIEL J. WALKOWITZ
New York University

DAVID THELEN. *Paths of Resistance: Tradition and Dignity in Industrializing Missouri.* New York: Oxford University Press. 1986. Pp. x, 321. $29.95.

In a famous line one of the characters in Clifford Odets's play *Awake and Sing*, produced in 1935, cautions against letting one's life be "printed on paper dollars." David Thelen discovers a similar struggle inherent in the history of Missouri from the end of the Civil War to the beginning of World War I. He portrays this history as a dramatic confrontation between traditional cultures and development, between family and community values and the new culture of growth and individual competition. The result is a fine study, a clear statement of position, and a provocative thesis—a book to read and contend with.

Thelen begins his study by dividing the Missouri world into two groups, resisters and developers. He sketches the local economies of the state and describes the variety of local cultures. The machine in this garden of agrarian provincialism is development pushed by "impatient Missourians" anxious to substitute market competition for provincial economies guided by custom, ethnic clannishness, and tradition. Beginning with early, "primitive" resistance, he documents the slow evolution of community resistance into a variety of efforts aimed at restoring local control, from local taxpayers' revolts to the more sophisticated "Missouri Idea" that sought to achieve state political power to unlock the grip of railroad and other monopolies on the economy of Missouri at the turn of the century. This, in turn, evolved further into the Progressive notion of regulation, something the author characterizes as abstract, remote, and basically subversive to the idea of real community control over the economy. This is where the book ends, with the compromise of thirty years of resistance and efforts to assert community control over forces and institutions much larger and more powerful than the community itself—as Thelen aptly puts it, of years aimed at shortening the distance between individuals and institutions.

There is no disputing the reality of resistance. The author offers an exhaustive description of a myriad of ways Missourians rejected aspects of the new society and sought to preserve old customs. He is also astute in demonstrating how traditional norms and organizations could evolve into movements of larger scope and more heterogeneous membership. He is at his best in the chapters on mutual-aid societies and the development of the "Missouri Idea." He is not unaware of the shadows on the communities he extolls—their racism, ethnic exclusivity, and intolerance. Yet he finds in them the substance of a modern ethics and the possibility of a culture not yet realized.

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What is controversial about this work is where the author chooses to be inclusive and exclusive. Listed among the resisters to development are Jesse James, Scott Joplin, and children who played hooky from school. No doubt these characterizations contain some truth, but Thelen avoids any alternative explanations of such phenomena and thereby the complexity and contradictions in this sort of evidence. There is probably more to James, Joplin, and errant schoolboys than resistance to the new economy. The problem is that not every form of misbehavior, mutual-aid society, or assertion of community control is resistance to economic change, nor is it necessarily progressive.

This leads me to the problem of exclusiveness and those Thelen leaves out of his story. Except for a few instances, women are not listed among the resisters, although their accompaniment is implied. Is this a problem of sources? What was life like for women in the traditional societies that fought modernization? Would the families and communities that blocked change seem so idyllic if women were put back into this garden?

Finally, Thelen's explanation for the failure of resistance is too short-winded. He rejects theories of cultural hegemony but leaves the impression that political confusion and the Progressive strategies of accommodation undercut resistance to change. Of course he credits racial and ethnic divisions too, but his explanation does ultimately seem to rest on political and strategic failures. This, however, does not explain why the developers won.

Perhaps these questions seem to go beyond Thelen's intention. But I ask them only because his book demands that we think seriously about this extraordinary era in Missouri history and the larger issues that his fine work raises.

JAMES GILBERT
University of Maryland

DONALD L. MILLER and RICHARD E. SHARPLESS. *The Kingdom of Coal: Work, Enterprise, and Ethnic Communities in the Mine Fields.* Philadelphia: University of Pennsylvania Press. 1985. Pp. xxii, 360. Cloth $35.00, paper $17.95.

The appearance of this work is timely. In the period when many of America's old industries are declining, a general survey of anthracite coal mining, one of the first industries to experience the full process of birth, growth, and decay is instructive. As this study by Donald L. Miller and Richard E. Sharpless reveals, the tale of anthracite is a tragic drama of human and ecological waste, from the difficult processes and working conditions to the exploitation of both the workers and the land, accompanied by the miseries of unemployment and poverty.

Miller and Sharpless have done a good job of pulling together previous research on the social history of the anthracite industry to produce the fullest study of the topic to date. Scholars will nevertheless be disappointed. The study was clearly designed for a general audience and beginning students and the authors have not informed their work with the most recent interpretations of labor, immigration, and social historians. The study is essentially a compilation of earlier studies of the anthracite industry to the 1920s. It sheds no new light on nor offers any fresh insights into the hundred-year story of hard coal.

The record of the industry certainly possesses its own extraordinary literary appeal, which the authors have captured: the early entrepreneurial efforts, the role of labor in production and marketing, the flamboyance of the Middle Field operators, the kaleidoscopic succession of immigrant settlements, the violent labor struggles, and the suffering by all in the region in recent years. Retold once again are the stories about the legendary Molly Maguires, the epic strikes at the turn of the century, and, especially, the dramatic Lattimer massacre.

The book's major shortcoming for historians, however, is its methodology—specifically, its refusal to go beyond its sources, which are largely secondary works in English. Even with the use of new oral histories, the work has little to add to its citations. An example of an opportunity missed is the treatment of East European immigrants. After a brief, informative section on Slavic customs, the authors eschew any deeper analysis of the ethnic family or community. In fact, virtually absent is any consideration of the internal structure, the social stratification, and the institutions of foreign settlements.

The treatment of the post-1920 period is particularly weak: the section on the 1920s is little more than an account of industrial relations, and that on the 1930s is merely a review of anthracite labor councils. Very little is said about the social life of the mining community. A final weakness is the book's design. Too many of the scores of photographs are undated and misplaced in the text. Furthermore, for such a large, proportioned work of 150,000 words the publisher's insistence on a paperback volume is unjustifiable. In sum, the people of the anthracite region will clearly be happier with this work than will professional scholars.

VICTOR GREENE
University of Wisconsin, Milwaukee

BRIAN GRATTON. *Urban Elders: Family, Work, and Welfare Among Boston's Aged, 1890–1950.* Philadelphia: Temple University Press. 1986. Pp. xv, 232. $32.95.

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By tracing continuities and changes in the meanings and experiences of growing older in America over a broad sweep of time, the first books in historical gerontology established a serviceable survey and intriguing set of hypotheses with which to reconstruct the elderly's place in the national experience. In contrast, Brian Gratton provides a case study in which he deliberately limits his focus to the shifting demographic, socioeconomic, and political status of older men and women who lived in a major ethnic and industrial center during the first half of the twentieth century. He thus is able to build on (and challenge) current interpretations as he provides evidence for more precise linkages between the social (welfare) history of aging and the political economy in which successive cohorts of older persons lived.

"On the whole, the Boston case reveals milder economic effects than predicted [against old age] than politics, and no inevitability at all [in developing a welfare system]" (p. 174). Gratton argues that prior to 1930 most older people were neither marginal, obsolescent, nor impoverished. Changes in the economic structure coupled with the need to placate Irish working-class voters, however, led Massachusetts to enact old-age benefits before the advent of Social Security. These benefits succored an important minority of public wards, served as a retirement wage, and protected families from the covert dependency of older members.

Gratton's book is revisionist in every sense of the word. Historians should follow his lead and explore ethnic differences within the elderly population as assiduously as they probe the significance of race and gender. The impact of grass-roots politics on federal policies demands attention in order to assess the representativeness of Boston as a case study. I think that Gratton belabors the dichotomy he sees between "conventional" attitudinalists and structuralists: his tripartite division of the history of old age (preindustrial, industrial, and post-New Deal), after all, has a familiar ring. But I hasten to add that this book is required reading precisely for the vigor and rigor with which Gratton presents his case.

W. ANDREW ACHENBAUM
Carnegie Mellon University

ROBERT ANTHONY ORSI. *The Madonna of 115th Street: Faith and Community in Italian Harlem, 1880–1950.* New Haven: Yale University Press, 1985. Pp. xxiii, 287.

Some years ago the pastor of an Italian parish in Cleveland called my attention to the several statues of saints incorporated in the base of the high altar. He proudly explained that placing these remnants of Italian religious traditions in the reconstructed altar represented the final step in a campaign to lead Italian immigrants away from what he considered the irregular, embarrassing, superstitious, and even pagan faith that they brought from southern Italy. Firmly set in the base of the common altar, these figures lost their individuality and, with it, their capacity to command attention as powerful spiritual figures competing with the church for the immigrants' loyalty and financial support. They could no longer preside over annual festivals in their honor, financed and managed by lay societies. They had been captured and "Americanized" by the church, ending what churchmen called the "Italian problem."

Robert Anthony Orsi examines the Madonna of 115th Street, a particularly important figure of this type (one of only three in the Americas to have special papal recognition) at a time when she was an independent and powerful force in Italian Harlem. He takes the popular religion of the immigrants seriously and seeks the inner history of the Madonna through the meanings that believers invested in their devotions to her. In his history the "festa" becomes a sacred theater in which the people of Italian Harlem revealed their deepest values and understanding of their world. That world was encapsulated in a particular family culture Orsi labels the "domus."

The centrality of "la famiglia" in the Italian experience is widely accepted, but its nature is hotly debated. Orsi's description of the "domus" is an extreme statement of the view that the Italian family was characterized by a demand for extraordinary sacrifice of its members' individual interests. It is difficult to see how a family form that provided so little satisfaction, featured so much exploitation, and caused so much unremitting frustration, pain, and anger for all its members could persist for any length of time.

Although principal elements in this history of the Italian-American experience have been explored by others, Orsi makes important scholarly advances. Italian anticlericalism and the conflict between the American Catholic Church and Italian modes of faith and spiritual expression have received considerable study. This literature adopts the American churchmen's characterization of the "Italian problem" even when siding with the immigrants against the church. Orsi takes a more serious and sympathetic view of Italian popular religion and insists that it is a legitimate part of Catholicism in America.

Orsi uses three forms of documentation that speak to and for the Italian community in Harlem. He draws heavily on Leonard Covello's extensive interviews, reports, and biographies. Parish bulletins provided information and insight concerning the Madonna and her festa. In addition, the author did extensive interviewing for the book.

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Orsi has fashioned an impressive fusion of the inner histories of immigrant social and religious life, even if, as I suspect, the Madonna inspired more joy, satisfaction, and sense of God's providence among her worshippers than is revealed here. This book should serve as a model for the study of popular religion.

JOHN W. BRIGGS
Syracuse University

ROBERT C. NESBIT. *The History of Wisconsin.* Volume 3, *Urbanization and Industrialization, 1873–1893.* Edited by WILLIAM FLETCHER THOMPSON. Madison: State Historical Society of Wisconsin. 1985. Pp. xiii, 693. $30.00.

Robert C. Nesbit's contribution to the six-volume *History of Wisconsin* covers the brief period 1873–93, which the author suggests produced the greatest transformation of the state's economy ever. This third volume in the monumental series being produced by the State Historical Society of Wisconsin is, like its predecessors, packed with details of the state's rich history.

Nesbit has employed a traditional approach to state history, dividing his volume into three main parts entitled "The Economy," "Communities," and "Politics and Government." Because of its encyclopedic nature, the work is difficult to review briefly. Nesbit's selection and organization of materials do illustrate one of his major contributions, which is that Wisconsin's history during this period might serve as a microcosm of the nation's history. The similarities are legion: the state's farmers switched from small-scale grain farming to market-based farming, largely dairying; the state's population moved steadily from farm to city; unions fared poorly among the state's workers as industry grew in importance; employment of women increased, but wage disparity with men continued; Republicans dominated state politics, winning eight of eleven gubernatorial elections during this twenty-year period; and the state (and its peoples) emerged from these two decades generally better off than they had entered them.

Nesbit's book provides a valuable source of information for anyone seeking to know more about life in Wisconsin and the nation during these two decades. The book contains much statistical information on nearly every facet of the state's history. Despite the mass of details in this comprehensive account of such a short time period, the book is quite readable.

The chapters on agriculture and lumbering are among the best in the book. Nesbit notes the rural to urban movement and the switch from crop farming to dairying, which became the dominant activity on Wisconsin farms by 1890. His account of the logging business with its basic "cut and run" philosophy is a superb review of this sad chapter in the state's history. The section on making a living provides valuable statistics on how people lived and worked (and where), although it is not calculated to make the state's employers look generous. The information provided on the importance of immigrants to the state's labor force, religious outlook, and politics is also useful. Nesbit notes that the high concentration of foreign-born residents made the politicians act cautiously. The state's liberal suffrage provisions enabled immigrants to participate quickly, and the Republican majority soon learned not to take the immigrant votes for granted.

Less satisfactory are the sections dealing with Indians (Nesbit notes the numerous tribal variations and the inexactness of numbers as reasons for not providing more in this section), blacks (not many lived in the state), and labor. The state's political history is recounted in the last portion of the book with little new information included. Republicans dominated the political scene except after the Democratic sweep in 1890, which resulted from the controversy over the Bennett Law. The law, requiring schools to conduct instruction in English and parochial schools to report annually to the state, was opposed by the state's heavy German population. Once this law was repealed, Democratic ineptitude and the depression of 1893 brought Republicans back into power.

Nesbit asserts that, despite a reputation to the contrary, the state's government did work better than contemporary newspapers or later critics implied. And he concludes that "Wisconsin's people emerged from these years with a better chance at an education . . ., slightly healthier, worked shorter hours" in workplaces that were "generally safer" with public accommodations that "were meeting standards . . . and overseen by state agencies" (p. 647).

Complete with many photos and illustrations and a comprehensive essay on source materials, this book is a major contribution to the history of Wisconsin and a vehicle to a better understanding of the history of the United States during a period in which both state and nation were undergoing major transformations. The changes, chronicled in detail by Nesbit, assured that Wisconsin after 1893 would be markedly different from the youthful state that existed in 1873. The book is a worthy addition to the series of which it is a part and to the field of state history.

JUSTUS F. PAUL
University of Wisconsin, Stevens Point

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LALA CARR STEELMAN. *The North Carolina Farmers' Alliance: A Political History, 1887–1893.* (East Carolina University Publications in History, number 6.) Greenville, N.C.: East Carolina University Publications. 1985. Pp. xiv, 295. Cloth $19.95, paper $9.95.

Lala Carr Steelman's book is a traditional history of the politics of the Farmers' Alliance in one state, focusing throughout almost entirely on the leadership of the organization. It helps illuminate the political background of the emergence of the Populist party in the South.

Steelman begins with a discussion of the economic and political conditions in Redeemer-controlled North Carolina that led to the spread of the National Farmers' Alliance and Cooperative Union after 1887. Chapter 2 concerns the involvement of the alliance in the election of 1888 and the subsequent legislative session. Alliance candidates won a number of seats, but the Redeemers controlled the session, in general defeating initiatives of the alliance, particularly its efforts to regulate the railroads. Alliance legislators did, however, successfully support the Payne election law, designed to reduce the poor white and black vote and limit the possibility of federal supervision of state elections.

Chapters 3 and 4 discuss the senatorial and congressional campaigns of 1890 and the alliance's role in them, particularly in regard to the subtreasury plan, considered by most alliancemen in North Carolina, conservative and radical alike, as the key to the alliance program. The refusal of the Democratic party to support the plan began the split between the conservative alliance leaders, who wished to keep the alliance firmly attached to the Democratic party, and the more radical ones, particularly L. L. Polk, who flirted more and more with the alternative of creating a third party.

Chapter 5 covers the General Assembly of 1891, which members and candidates of the alliance dominated. This session produced a good deal more reform legislation than its predecessor, including railroad tax laws and a railroad commission, conservation laws, and increased educational spending. This legislature also acted to reduce black political power again and to prevent continued black migration out of the state.

Chapter 6 discusses the split within the alliance in North Carolina over the issue of the third party. Chapters 7 and 8 focus on the election of 1892, the first in which the Populist party figured. Most of the alliance leadership stayed with the Democratic party, which nominated Elias Carr, president of the state alliance, for governor. The Democrats' nomination of Grover Cleveland for president on a conservative platform did not change their minds, although apparently many of the rank and file went with the Populists. Steelman argues that politics, not the failure of the cooperative movement, destroyed the alliance in North Carolina. The organization could not survive this split in its membership.

The book is the product of thorough research in the relevant collections. Perhaps the most interesting part of the book is Steelman's discussion of the alliance-dominated legislature of 1891. She maintains that the secret of its success, as compared to its counterparts elsewhere in the South, lay in its active leadership and "the moderate nature" of the reforms it attempted (p. 165). In matters of reform, including its dealings with black North Carolinians, Steelman finds that almost all of the state's alliance leaders were moderate or conservative on almost all occasions.

BRUCE PALMER
University of Houston, Clear Lake

M. JAMES PENTON. *Apocalypse Delayed: The Story of Jehovah's Witnesses.* Toronto: University of Toronto Press. 1985. Pp. xvii, 400. $24.95.

Sprinkled throughout the history of Christianity are people who scrutinize biblical prophecies for precise scenarios of the future. What happens when their prophecies fail, when Jesus and the millennium do not arrive on schedule? Do they admit their mistakes and promise never to make them again? Rarely. Whereas a few apocalyptic groups disband in disgrace, most simply make adjustments and keep going. No group in modern times has been more successful at surviving their prophetic disconfirmations than have the Jehovah's Witnesses.

M. James Penton has written what may be the most penetrating study of this movement to date. A fourth generation Jehovah's Witness who was disfellowshiped in 1981 for heresy, Penton has written a carefully researched and nuanced analysis that both benefits and suffers from his personal experience with the Jehovah's Witnesses.

Penton divides his book into three sections (historical, conceptual and doctrinal, sociological and psychological) because, he says, previous studies mixed "academic genres" in confusing ways. That may be so, but, to make his story understandable, Penton must mix them too. One cannot relate the history of the Jehovah's Witnesses without delving into doctrine any more than one can explicate their beliefs without showing how they changed over time. Consequently, the three sections overlap and are often redundant. When Penton does compartmentalize, he sometimes leaves out important information that makes the story hard to follow, especially for the uninitiated reader.

Despite some confusion in organization, Penton knows how to tell a good story. In the late nine-

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teenth century, when many evangelicals were scanning the Scriptures for clues to Christ's coming, Charles Taze Russell founded the Bible Students' Association. At first similar to other evangelical millenarians, the members of the association soon went their own way. They adopted an Arian christology and developed a rather unique way of calculating events of the end of time. The latter led them into a series of prophetic disconfirmations about the return of Christ and the end of the world. After a failed prophecy in 1917 and the death of Russell, the association changed its name to the Jehovah's Witnesses and, in Penton's view, took a turn for the worse. Instead of abandoning their mistaken prophetic system, the Jehovah's Witnesses changed their story and turned inward.

Under Russell, the association was a devout, Bible-oriented group that retained much local autonomy and permitted differences of opinion. But Russell's successor, Judge Franklin Rutherford, emphasized strict conformity and the centralization of all power in the Watch Tower Society. The resulting "theocracy" controlled nearly all aspects of the lives of Jehovah's Witnesses, thereby becoming authoritarian and totalitarian. The society's Brooklyn headquarters ordered Jehovah's Witnesses to become door-to-door proselytizers and to keep away from "worldlings" as much as possible. Since the 1970s the Watch Tower Society has experienced growing internal dissent, which it has controlled by discrediting and disfellowshiping those who raise questions.

Penton is a judicious historian. He has done his homework well and deals extensively with primary sources. He knows the latest research on the Jehovah's Witnesses and handles it critically and fairly. He deftly explains their doctrine, unravels their organizational structure, and analyzes internal changes that led many Jehovah's Witnesses to defect since the mid-1970s. He tells why people join the movement and then stay or leave. With only rare lapses, he avoids polemics. Although he obviously has strong negative feelings about the Watch Tower leadership, he does defend the movement against unwarranted and inaccurate accusations. In fact, Penton's personal involvement with his topic is the book's greatest strength and, occasionally, its greatest weakness. As a former insider, Penton has cognitive and affective expertise. For example, his discussions on the Jehovah's Witnesses' view of morality, social taboos, family life, and higher education have the ring of authenticity and authority that comes from active participation. But sometimes Penton's "inner history" lacks the careful documentation that historians require. When he describes the mood of the movement, the private reactions of Jehovah's Witnesses to the leadership's directives, and the subtle pressures against those who seek higher education, he fails to cite many concrete sources. Some readers may readily absolve him for such omissions (after all, they do give the book a rich affective texture), but scrupulous historians may feel a bit uneasy about them.

All in all, this is an excellent book and required reading for those interested in Jehovah's Witnesses. Penton's special perspective provides a scholarly inside look at a fascinating and persistent example of modern millenarianism.

TIMOTHY P. WEBER
Denver Seminary

JACOB RADER MARCUS and ABRAHAM J. PECK, editors. *The American Rabbinate: A Century of Continuity and Change, 1883–1983*. Hoboken, N.J.: KTAV. 1985. Pp. 261. $20.00.

Between the 1880s and the 1980s the American Jewish community multiplied in size, changed its demographic pattern, and, slowly at first, then more rapidly in the second fifty years, moved into the mainstream of American life. As a result, "Jewish Studies" has come of age as a "separate but equal" interdisciplinary field. This book is a useful addition to the literature of this field.

Focusing on the rabbinical authors of the Orthodox, Conservative, and Reform movements, Jacob Rader Marcus and Abraham J. Peck planned to explore the distinctions among the three major Jewish denominations in the past century. They chose to start with 1883, the year of the first graduating class of Hebrew Union College, the Reform seminary in Cincinnati. Four years later, in 1887, the seed of what later became the Jewish Theological Seminary in New York City was planted. Still later in the same decade the Association of American Orthodox Hebrew Congregations, ultimately the nursery of the Orthodox movement, was founded.

The detailed and illuminating studies constituting the book were produced by three scholars: Jeffrey S. Gurock, Abraham J. Karp, and David Polish. Jonathan D. Sarna wrote the introduction. The book attests the editors' intention to stress the pluralism of American Jewry. The contributors, however, go beyond this intent to display and illuminate the internal pluralism of each of the denominations. Setting the scene, Gurock divides the Orthodox rabbis into "resisters"—those who resist any modification of the East European pattern of Jewish life—and "accommodators"—those who assert the need for some adjustment to the novel American situation.

Polish sees in Reform Judaism similar concern for "the changing and the constant," exemplified in the turning away from the earlier radical universalism

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of the Central Conference of American Rabbis to more particularistic positions, such as supporting Zionism, stressing Hebraic studies, and reemphasizing rabbinic legal authorities in training programs. Even if the "turning" developed, in part, because of the reviving anti-Semitism of the 1920s and 1930s, the radical change in statements of the conference within a decade is still remarkable. Henry Ford's *Dearborn Independent* in the 1920s, the activities of Charles E. Coughlin in the 1930s, and many less-familiar expressions of American anti-Semitism anticipated the larger reaction stimulated by the rise of Hitler. Despite this, Polish concludes, "Today's Reform is neither universalistic nor particularistic but Jewishly ecumenical" (p. 243).

Karp views the Conservative rabbinate as "dissatisfied but not unhappy" with their lot. He finds that, although an intermediate position between nineteenth-century Reform Judaism and immigrant Orthodoxy was tentatively sketched in the latter part of the nineteenth century, the Conservative movement did not develop until Solomon Schechter's tenure as head of the Jewish Theological Seminary (1902–15). Schechter had a scholar's readiness to allow wide diversity of belief, provided that scholarly discipline was maintained. No doctrinaire, he "laid the foundation for the ideological diversity which has marked the Conservative rabbinate, bound together more by institutional loyalty than ideological agreement" (p. 115). Thus, Conservative Judaism also cuts a wide swath; pluralism is at the heart of the movement. As Sarna wrote, "In Conservative Judaism, both traditionalist and progressive rabbis could lay claim to harboring the true spirit of the movement" (p. 3).

The breadth attributed to each position suggests that some overlap must occur at the seams. The most traditionally inclined Reform rabbis could well stand to the right of the least traditional Conservative rabbis, whereas the most traditional Conservatives might stand to the right of the most liberal Orthodox rabbis.

This excellent book will help any reader understand why so much range and variety exist and, especially, why they exist in the United States.

JOSEPH L. BLAU
Columbia University

DAN A. OREN. *Joining the Club: A History of Jews and Yale.* (The Yale Scene; University Series, number 4.) New Haven: Yale University Press, in cooperation with the American Jewish Archives. 1985. Pp. xiv, 440. $29.95.

For Jews who reached college age in the 1940s, anti-Semitism on campus was a fact of life. Jewish teenagers understood that words like "geographical balance" and "clubbable" masked practices designed to limit a Jew's acceptance into certain universities and collegiate societies. They knew before they read Ludwig Lewisohn that positions in academia, particularly in the humanities, were generally beyond their grasp. I was told by an admissions officer at a prestigious women's university that her institution catered to girls outside New York City who were "not fortunate enough to have a Hunter College." A concerned professor advised Jewish female students from New York to forget about college teaching: they had three strikes against them at the very outset.

As did most respected private universities, Yale lived by the rules of discrimination from the 1880s to the 1960s. That is the core of Dan A. Oren's richly researched and well-written book that charts the history of Jews at Yale from the first Jewish student (Moses Simons, class of 1809) to the offer of the university presidency in 1977 to Polish-born Henry Rosovsky. Oren shows how social anti-Semitism permeated the institution during the Gilded Age and, after World War I, hardened into fixed limits on the admission of Jews. Similar restrictions characterized the policies of Yale's professional schools, recruitment of faculty, and student societies. Discrimination held fast in open or veiled form until a confluence of forces after World War II—popular revulsion against racism, the appearance of veterans on campus, the decline of the fraternities, the leadership of A. Whitney Griswold and Kingman Brewster—restored meritocracy to the university.

The author's purpose is not to explore the entrenchment or significance of social anti-Semitism. On those matters he draws on the conclusions of John Higham and E. Digby Baltzell. Oren's focus is on the operations and patterns of institutionalized discrimination. He indicts Yale for succumbing to the values of materialism, conformity, and caste and for ignoring a university's responsibilities to elevate the intellect and to encourage pluralism. Nevertheless, his assertion that discrimination was Yale's problem and not a Jewish problem is belied by his own evidence. Even the exceptional few who "made it" wrestled uncomfortably with their identity and identification as Jews. They, along with the many whose Jewishness barred the way to acceptance, were still beyond the pale of normality and respectability set by the bigoted guardians of a closed society.

Capping the intrinsic value of this engrossing study is the fact that Yale opened its files to Oren and, as the publicity surrounding the book's publication revealed, blessed the finished product. A public *mea culpa* on the part of a reformed bigot is a

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singular phenomenon in the annals of any minority's history.

NAOMI W. COHEN
Hunter College
City University of New York

MICHAEL BEZILLA. *Penn State: An Illustrated History.* University Park: Pennsylvania State University Press. 1985. Pp. xii, 415. $39.50.

Michael Bezilla has struck a middle ground between a coffee-table pictorial collection and full-scale narrative in this well-written and surprisingly candid illustrated history. Although he has made extensive use of internal documents, newspapers, campus publications, and interviews, the author has not delved into available manuscript collections and personal papers of key figures in Penn State's history. The result is a comprehensive survey limited in its depth of analysis.

Born as the Farmers' High School in 1855 on the eve of the Morrill Act that would shape the course of its history, Penn State is today one of the largest public universities in the United States. For much of its existence the "cow college" in central Pennsylvania compared unfavorably with the state's major private universities that continuously received state allocations. Although the Pennsylvania State College (its name from 1874 to 1953) received all of the state's land-grant funds, it long suffered from an identity crisis as it sought to establish its niche as the state university of Pennsylvania.

The small agricultural school developed a different focus with a growing emphasis on engineering education under President George W. Atherton (1882–1906). During the administrations of John M. Thomas (1921–25) and Ralph D. Hetzel (1927–47), the broadening of academic programs and significant expansion of research signaled that the Pennsylvania State College was becoming a university in fact if not in name. In the land-grant tradition Penn State also established a strong record of service to the Commonwealth through its Agricultural and Engineering Experiment Stations and associated extension programs. The first branch campus "undergraduate centers," begun in the 1930s for first- and second-year baccalaureate work, evolved into today's extensive Commonwealth Campus System, which accounted for nearly 40 percent of the university's undergraduate enrollment by the early 1970s.

The presidency of Milton Eisenhower (1950–56) brought important national exposure to Penn State, and the 1950s also inaugurated the tenures of coaches Rip Engle and Joe Paterno, who established the school as a regional and national power in football. Under Eisenhower's successors—Eric Walker, John Oswald, and the current president, Bryce Jordan—Penn State has continued to make significant strides in upgrading faculty, expanding research, and extending academic programs. The opening of the Hershey Medical Center in 1967 represents one example of these trends. Penn State's history parallels that of most large land-grant state univerities, and this volume adds to the literature on these institutions by updating and superseding Wayland F. Dunaways's *History of the Pennsylvania State College* (1946). Bezilla has produced an attractive and interesting internal history of the school. Larger external questions, such as the cultural impact of the school in Pennsylvania and the importance of Penn State's contributions to agricultural and industrial development in the state and nation, are addressed only superficially. The author has accomplished the task set before him by his sponsor; it is unfortunate that the agenda was not more ambitious.

AUGUST W. GIEBELHAUS
Georgia Institute of Technology

ROBERT C. MCMATH, JR., *et al. Engineering the New South: Georgia Tech, 1885–1985.* Athens: University of Georgia Press. 1985. Pp. xii, 560. $37.50.

This book chronicles the troubled history of an institution "chartered in the 1880s as part of a strategy to harness public and private resources in support of public growth" (p. 439). An official centennial publication written by Robert C. McMath, Jr. and other members of Georgia Tech's history department, it claims to offer institutional history framed by intellectual, economic, and political crosscurrents both regional and national in character.

Influenced profoundly by a nineteenth-century "commercial shop" model of technical education, Georgia Tech's past is described as inexorably tied to Atlanta's struggles to industrialize, the inchoate emergence of Georgia's system of higher education, and the politics of a state that, historically torn by racism and poverty, seldom provided adequate resources. Like other books of its genre, this one considers the struggles and policies of presidents, the character of student culture, and the evolution of the curriculum. Subthemes include Georgia Tech's tortured attempts to define itself amid interference by politicians, academic mediocrity, administrative paternalism, and a white, male-dominated, middlebrow campus culture. Although strong leadership and Atlanta's current prosperity have recently improved Georgia Tech's programs and standards enormously, it is still, the book's authors suggest, a regional institution highly dependent on the erratic economic condition of the South and, as

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a latecomer to the post-World War II academic revolution, the budgets and research priorities of the Department of Defense.

As commissioned institutional histories go, this one is forthright and unsettling. The narrative is, however, flawed. The ideology and realities of the New South are described superifically and unevenly. Few statistics and little other information on economic and political conditions in Atlanta and the South are included, and data on Georgia Tech's resources, students, and regional influence are provided sporadically. Other southern technical institutions receive no notice; northern ones are but mentioned in passing. The book suffers from a total lack of commentary on the history of southern higher education, about which there is a growing and sophisticated literature of which the authors seem unaware. Too often, the authors devote far more attention to athletics than to the issues that give the book so much promise at its beginning. It becomes difficult to determine whether, and to what degree, the founders' philosophy, the flawed ideology of the New South it was to serve, institutional conditions, or the political and economic climate of Georgia account for the way in which Georgia Tech's history evolved.

RONALD K. GOODENOW
Boston University

JOHN M. O'DONNELL. *The Origins of Behaviorism: American Psychology, 1870–1920*. (American Social Experience, number 3.) New York: New York University Press. 1985. Pp. xii, 299. $40.00.

Much interesting work in American intellectual history has recently been done on the histories of individual academic disciplines, and John M. O'Donnell's well-researched study now belongs on the same shelf with Bruce Kuklick's on philosphy and Fred Matthews's on sociology. O'Donnell's study is superior to these works in that it attempts to cover the whole American academic scene but inferior in that its language is often cryptic and opaque.

O'Donnell has a revisionist view. The influence of German psychologists, in particular of Wilhelm Wundt, on early American psychologists has been exaggerated, and that of the British Darwinian Alexander Bain and of phrenologists have been underemphasized. O'Donnell sees behaviorism as emerging from strong practicalist pressures and describes the discipline of psychology immediately after 1900 as closely allied with education and such utilitarian ventures as mental testing, learning theory, and advertising. Thus, John B. Watson's behaviorism was not so much a platform of metaphysical revolt as a program congenial to down-to-earth applied science, often in collaboration with public school systems, especially in midwestern state universities, or with corporate America. The ideal of pure science had some initial force in the 1880s and early 1890s, but it tended to evaporate later on.

The book is less successful in describing the philosophical controversies that initially affected the study of psychology than in recounting the later practicalist pressures. O'Donnell neglects the issue of a growing demand for the autonomy of psychology as a discipline, involving separation from departments of philosophy. By emphasizing the victory of utilitarianism, O'Donnell downplays the importance of purely intellectual struggles. Early concern that the teaching of psychology avoid materialistic implications is emphasized, but its fading away after 1895 is not directly explained. A great opportunity for study of the process of secularization in the American intellectual community has thereby been missed. The changing content of psychological teaching is also neglected. What did an undergraduate in a beginning psychology course really have to "learn" in order to pass? Instead, O'Donnell concentrates on such figures as William James, G. Stanley Hall, and George Trumbull Ladd but unfortunately does not make them come fully alive; the bits and pieces he presents about each of them do not quite jell into a vivid series of portraits. Throughout the book one frequently has a sense of skipping too abruptly from one insight or one professor of psychology to another.

Yet the insights can at times seem profound. The book contains much material of the highest value about the academic milieu and the discipline of psychology in the earliest decades of the university. Moreover, in the best traditions of pure science O'Donnell manages to maintain a tone of descriptive distance from his subject, without crudely attacking the people involved. O'Donnell is, from one point of view, chronicling the triumph of a manipulative mentality, shared by a group of professors and their nonacademic clients, yet he writes about it dispassionately. One result is that the issue of manipulation, familiar to our own period but not so much to theirs, gets muted.

LAURENCE VEYSEY
University of California, Santa Cruz

JOHN CLENDENNING. *The Life and Thought of Josiah Royce*. Madison: University of Wisconsin Press. 1985. Pp. xv, 447. $27.50.

At the end of the nineteenth century, Josiah Royce dominated American philosophy. By his death in 1916, the pragmatic and realistic attack led by his friend and colleague William James and joined by many of his own students had pushed Royce's

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philosophy to the margins, where it has remained. John Clendenning, editor of *The Letters of Josiah Royce* (1970), offers a study of the man and his thought that demonstrates Royce's former importance and argues for his continuing significance.

Clendenning traces Royce's life and career from his birth in frontier California in 1855. Educated at Berkeley and Johns Hopkins, with a German sojourn, Royce became one of the first generation of professional philosophers. Following his appointment at Harvard in 1882, Royce became the foremost exponent of absolute idealism in the United States and a leading investigator of new developments in logic. At the peak of his powers with the publication of *The World and the Individual* (1899–1901), Royce thereafter witnessed the decline of his influence. Nonetheless, his thought took new directions exploring the ideas of loyalty and community before his death at age sixty-one.

Clendenning relates the development of Royce's thought to his life and psychological development. Relying loosely on the theories of Erik Erikson, he sees Royce's life as a search for identity whose strong echoes can be found in the philosophical writings. Always a loner, Royce's personal life was marred by a sense of isolation and alienation, an apparently loveless marriage, and personal tragedy, including the insanity of his oldest son. Although Royce universalized and abstracted his experience, Clendenning makes a plausible case that the philospher's thought was partially grounded in his life. Clendenning, however, is inclined to push his psychological interpretation too far, as when he speculates that in one essay Royce was "unconsciously anticipating the double shock" of the deaths of his son and of William James eighteen months later (p. 335). The psychological insights do help us to understand the relationship of the man to his thought, although, as Bruce Kuklick demonstrated in *Josiah Royce: An Intellectual Biography* (1972), the philosophy can stand alone.

Clendenning summarizes the major arguments of Royce's essays and books and places them in the context of his career. Acknowledging the present obscurity of Royce's thought, he argues that Royce has some continuing significance as an originator of pragmatism, as an early advocate of ego psychology, and as a herald of twentieth-century alienation. Historically interesting, these observations are not sufficient to spark a major revival of Roycean thought.

Clendenning's biography is well written and makes accessible to a sympathetic reader the core of Royce's thought. The style of citation (notes gathered at the back by page number) is, however, cumbersome and annoying. Clendenning argues convincingly for Royce's relevance to American thought and culture at the turn of the century and for the connections between felt experience and Royce's philosophical writings, but he fails, ultimately, to make Royce an indispensable thinker for the late twentieth century.

DANIEL J. WILSON
Muhlenberg College

JAMES E. CEBULA. *James M. Cox: Journalist and Politician.* (Modern American History.) New York: Garland. 1985. Pp. 181. $30.00.

James M. Cox (1870–1957), Ohio newspaperman and publisher, served in the U.S. House of Representatives (1909–13) and as governor of Ohio (1913–15 and 1917–21). His political career reached its climax with his nomination as the Democratic party's presidential candidate in 1920. His newspaper empire included the *Dayton Daily News*, the *Miami* (Florida) *News*, and the *Atlanta Journal.*

James E. Cebula sees Cox as an urban leader concerned with adjusting "the values of middle class democracy" to "the pace and rhythm of urban life" (p. 11) and his nomination not as a shift to the right but as a victory of urban over rural Democrats.

Cox was a leader of the Progressive movement in Ohio, and his newspapers championed Progressive causes and urban improvements. When he was governor, the "Cox laws" became the centerpiece of state-level Progressivism in Ohio. He favored welfare capitalism, labor unions, reforms to eliminate "social and political unrest" (p. 37), and governmental efficiency.

Like some southern Progressives, he was also a frank racist who thought government was best when in the hands of Anglo-Saxons. He supported segregation, opposed Asian immigration, and, as governor during the war, suppressed the teaching of German, which he called "part of a conspiracy formed long ago by the German government in Berlin" (p. 91).

As a presidential candidate, Cox ardently championed U.S. membership in the League of Nations and later sought to make it the leading issue in 1924. He opposed Franklin D. Roosevelt's nomination in 1932 but supported the New Deal.

This short work is a useful supplement to Cox's autobiography, but it is not exhaustive.

WESLEY M. BAGBY
West Virginia University

DAVID E. KYVIG, editor. *Law, Alcohol, and Order: Perspectives on National Prohibition.* (Contributions in American History, number 110.) Westport, Conn.: Greenwood. 1985. Pp. xiii, 218. $35.00.

National prohibition presents a familiar, yet controversial and little-studied topic for research. It offers

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significant unexplored territory for nearly every type of historian, regardless of specialty. Social historians can begin with the subject of drinking and its links to urbanization, ethnicity, social mobility, and changing values and experiences. Political historians, whether armed with notecard or microchip, can assault the peaks of top-level politics in both the federal government and the states, analyze the great shift in mass voting between the prohibition referenda of the 1910s and the elections of the repeal conventions in 1933, or trace the links between these processes and the partisan realignment that produced the New Deal coalition. A vast plain of near-virgin territory awaits legal and constitutional historians. For the business historian, the fate of the only major industry to be destroyed and revived by federal action beckons the prospector. As a bonus for the scholarly explorer with a taste for readership, few episodes in American history can match national prohibition's hold on the public imagination.

Both the importance and the fertility of the prohibition frontier are indicated by the essays published in this useful volume that was edited by David E. Kyvig. The principal impetus for the book was a conference sponsored by the Delaware Humanities Forum and the Eleutherian Mills Historical Library in April 1983 to commemorate the fiftieth anniversary of repeal. This conference, like others dealing with alcohol and temperance, attracted scholars from a wide range of disciplines as well as citizens and officials concerned with public policy; although most of the papers published here were written by historians, authors also represent political science, the law, and alcohol studies. Of the eleven essays, eight are revised versions of papers given at the conference, and the remaining three were prepared afterward by conference participants. Mark Lender's well-informed contribution surveys research needs and opportunities. Although the other ten papers are mainly exploratory in nature, taken together they suggest national prohibition's powerful impact on diverse areas of American life. For example, Nuala Descher argues that prohibition, by undermining the United Brewery Workers, destroyed a potent force for radicalism and industrial unionism. According to Paul Murphy, the excesses of the enforcement of prohibition contributed to the modern formulation of the concept of civil liberties. Rayman Solomon's careful study of the U.S. Court of Appeals for the Seventh Circuit finds that national prohibition introduced the lower federal courts to supervision of the federal bureaucracy, a task that would thereafter take on great importance. And Mark Haller traces the careers of bootleggers of the 1920s through their creation of America's postrepeal liquor distribution network to the building of Las Vegas. Whether national prohibition in fact played the causal role attributed to it in these and other essays must be determined by future research. As a stimulant to such research, this book performs its task admirably.

JACK S. BLOCKER, JR.
Huron College

DON H. DOYLE. *Nashville since the 1920s*. Knoxville: University of Tennessee Press. 1985. Pp. xvii, 330. $23.50.

With this book Don H. Doyle has completed his two-volume study of Nashville, the "Athens of the South," from 1880 to its bicentennial celebration in 1980. Like the first volume, which ended in 1930, this work is gracefully written and provides a detailed and thoughtful assessment of social and political developments in this city of the New South.

The history of Nashville during the past sixty years is told within the context of social tension between regional traditions and urban modernity. Doyle argues that "in Nashville the contest was at once more sharply defined and more ambiguous in its outcome" than in other southern cities (p. xiv). Even within this framework Doyle found divisions. Thus, several country music singers modified their style to achieve greater commercial success, and the intellectual community divided into two groups: those who embraced the agrarian movement and the preservation of the past and those who sought modern political reform and social change.

Doyle portrays Nashville as a community with many segments that contributed to a unified whole. The glue that held the enterprise together was, in Doyle's view, the religious faith that permeated the city "amid the vast changes in Nashville's economy, politics, and race relations in the decades following the 1940s" (p. 178) and the remarkable continuity in political leadership. In the political sphere urban boss Hilary House governed the city for twenty-one years, during which his organization provided social and economic services on an informal basis. When House relinquished power in the 1940s, the reformers expanded the functions of government along the lines of the welfare state. Even business leaders who had once advocated a limited notion of "good government" came to accept a comprehensive and centralized government that advanced the general welfare of the community. Doyle believes the modernization of Nashville culminated with the consolidation of county and city governments and the desegregation of the community. Neither came easy because both developments went counter to strong traditions.

Doyle has written a fine book in which he deftly handles a variety of issues, including modernization, urban machines, religion, music, and race relations.

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This study makes for good southern and urban history.

DAVID R. COLBURN
University of Florida

CHESTER H. LIEBS. *Main Street to Miracle Mile: American Roadside Architecture*. Boston: Little, Brown, for the New York Graphic Society. 1985. Pp. x, 259. Cloth $40.00, paper $19.95.

The commercialization of the roadside and the evolution of roadside commercial architecture are aspects of our automobile culture that have remained almost entirely neglected by historians. Historical archives are almost devoid of photographs and other documentation of the massive transformation of American open space into commercialized roadside strips since the turn of the century. Surviving structures that embody this transformation are rapidly being bulldozed to make way for still more modern forms. Chester H. Liebs makes the first notable serious attempt to redress this neglect of a commonplace but important phenomenon. He concludes that "there is much to be learned from reading the American roadside" and that "the significance of this staggering environmental transformation is still just beginning to come into focus" (p. 227).

Liebs begins by detailing the decentralization of retail trade from Main Street out along so-called taxpayer strips adjacent to streetcar lines, the revival of wayside retail business accompanying improved roads, and the growth of automobile tourism. By the 1920s this had led to a renaissance of roadside commerce. The final stage in the evolution of the roadside commercial strip came with the creation of the "Miracle Mile," a result of the proliferation of superhighways and regional shopping centers after World War II.

Roadside architecture necessarily came to convey at a glance to speeding motorists the commercial functions of buildings. The architectural styles that accompanied the commercial transformation of the roadside evolved from the dominance of domestic, "fantasy," regional, and historical imagery in the 1920s through various "modern" architectural styles from the 1930s into the post–World War II period to contemporary "environmental," "old building," and "high-tech" looks. After delineating this general evolution of roadside architectural styles, Liebs provides separate chapters on the development of and changes in the architecture of automobile showrooms, gasoline stations, supermarkets, miniature golf courses, drive-in theaters, motels, and restaurants.

Much valuable information on the roadside commercial strip and its architecture is made available for the first time. Liebs mines not only the standard secondary works on a wide range of specialized topics but also a vast body of heretofore-neglected articles in the periodical literature of the day. His synthesis is lucid, concise, and original. The text both explains and is immeasurably complemented by 17 color and 252 black-and-white illustrations. Many of these illustrations are original photographs taken by the author in over twenty thousand miles of travel doing field research.

These are considerable contributions. Yet Liebs falls short of realizing his overall objective. Although he gives due attention to the impact of broader economic and social changes on the evolution of the roadside strip and its architecture, the converse impact of the transformation of the roadside on American life remains largely an undemonstrated assertion. The result is that one comes away from Liebs's "reading of the American roadside" with at best a hazy notion of "the significance of this staggering environmental transformation" (p. 227). One hopes that future work on the subject will bring this transformation into focus.

JAMES J. FLINK
University of California, Irvine

FRANK COSTIGLIOLA. *Awkward Dominion: American Political, Economic, and Cultural Relations with Europe, 1919–1933*. Ithaca: Cornell University Press. 1984. Pp. 381. $27.50.

Here is an impressive attempt to view American-European relations during the fifteen years after World War I in their broadest and most comprehensive sense. Indeed, by devoting unusual attention to cultural as well as economic factors, Frank Costigliola has succeeded in identifying and tracking the interconnecting strands of this complex relationship to a much greater extent than have earlier scholars. In so doing, he has demonstrated that the world war, a rapidly developing domestic economy, and new cultural prestige abroad (modernization had come to be seen as "Americanization") combined to give the United States "important, but not unlimited, power in Europe" (p. 15), thus establishing what he calls an "awkward dominion."

Such predominance did not at first prevent American leaders from being sorely frustrated in achieving a peace settlement compatible with their hopes for gradual political change and orderly capitalist growth. Only with the Dawes Plan of 1923 after European defiance regarding security, reparations, and war debts had led to increasing international turmoil, was Washington able sufficiently to focus its influence to bring about a revision of the

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Treaty of Versailles largely in line with its own conceptions. Even then U.S. intervention was indirect and overly sanguine, relying on private financial tools and unofficial diplomacy to minimize both foreign responsibilities and domestic divisions while depending on continued international prosperity to pay the substantial costs. Furthermore, America unintentionally undermined the international economy by applying pressure to Europe to return to the gold standard, to balance budgets, and to pay debts—"policies that deflated the European economies while limiting opportunities for European economic expansion" (p. 265). If the American "system" functioned successfully for a few years, its formulas ultimately proved inadequate to cope with a major downturn in the world economy and an acceleration of German revisionism. In truth, the time had long since passed when the United States could effectively manage either its own order or that of Europe with the informal and largely economic methods on which it had relied in the past. By 1933 even American cultural influence was being seriously eroded as "the Nazis and the Soviets demonstrated that technological modernization was easily separated from the rest of the Americanization process" (p. 24).

Although generally plausible and exciting, Costigliola's argument is also burdened by problematic aspects. Chief among these is the author's repeated tendency to attribute more foresight and manipulative skill to the Republican leaders of that decade than these men conceivably could have possessed. Herbert Hoover and Charles Evans Hughes in particular assume almost god-like proportions, perfecting and masterfully employing those "new techniques in communication and advertising . . . [that], coupled with semiprivate diplomacy, offered effective means to mollify public and congressional opinion" (p. 57). European statesmen, by contrast, seem hardly present in the story at all; the French are especially noticeable by their absence in deed and in document. Behind all this, of course, the reader senses the significant presence of Costigliola's ideological perspective, a moderate radicalism of sorts that leads him to see the public at large as acted on by the elite (and ordinary nations by the dominant nation) and to emphasize economic factors much more than military, political, and diplomatic ones. Nevertheless, and despite any clash of world views, Costigliola's version of international relations in the 1920s does offer a better introduction to the variety of interacting factors than we have hitherto possessed. It will take its place alongside Melvyn Leffler's *The Elusive Quest* (1979) as a standard survey of the field.

KEITH L. NELSON
University of California, Irvine

NANCY L. ROBERTS. *Dorothy Day and the* Catholic Worker. Albany: State University of New York Press. 1984. Pp. 226. Cloth $36.50, paper $12.95.

Even before the death of Dorothy Day in November 1980, the Catholic Worker movement, which she cofounded with Peter Maurin in 1933, was drawing considerable scholarly interest. William Miller, Mel Piehl, and Marc Ellis have produced book-length studies of Day, Maurin, and the movement, and a number of excellent doctoral dissertations on Catholic Worker ideas have been completed. Nancy L. Roberts now adds to that growing corpus a careful study of the *Catholic Worker* and its editor and most frequent contributer, Dorothy Day. Published more or less regularly for over fifty years, the *Catholic Worker* has long been recognized as an unusual radical publication. Always attractively edited and illustrated, its writing sharp and crisp, the *Catholic Worker* has lasted where all others of its kind have not, even though it costs today, as in the beginning, a penny a copy and a quarter a year. Roberts surveys the remarkable history of this newspaper and attempts to explain some of the reasons for its influence and enduring appeal.

Roberts locates the history of the *Catholic Worker* within the category of advocacy journalism. Its point of view, a unique blend of orthodox Catholicism, Christian communitarianism, and pacifism, has always been clear and consistent. Using techniques of content analysis, Roberts demonstrates this consistency, as well as the persistent balance among articles covering the activities of the movement, reports on social issues, spiritual writing, and essays. Although its opinions were never in doubt, the *Catholic Worker* was a good newspaper and Day an excellent journalist. The newspaper featured reports from the field, covering strikes and social movements in the United States and, later, around the world. At the same time it introduced its readers to the most interesting and creative thinkers in the church. It bound the unorganized Catholic Worker movement together with essays on ideas of the Catholic Worker movement, columns by Day, and information on houses of hospitality and the activities of well-known Catholic Workers. Day and the other editors paid careful attention to format, writing style, and graphics, aided by the uniquely attractive art work of Ade Bethune, Fritz Eichenberg, and Rita Corbin. The simplicity of the basic Catholic Worker philosophy, the skills of the newspaper's writers and editors, and the consistency of its commitment—all held unflinchingly through depression and prosperity, war and cold war—help account for the paper's longevity.

Perhaps Roberts's most notable contribution is her careful examination of Day's role on the paper. Although many have noted Day's leadership in

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movements for peace and social justice and have appreciated the influence of her religious beliefs, few have taken seriously her own insistence that journalism was her vocation. She was an excellent editor and reporter, as Roberts demonstrates, and as prickly about criticism of her writing or changes in her material as any reporter would be. Most important, she was a literary figure whose writing grew increasingly graceful and powerful as the years went on. She combined careful attention to the details of daily life with a passion for the things of the spirit, and she has left a literary legacy whose popularity has continued to grow in the years since her death. Roberts has made skilled and comprehensive use of the files of the *Catholic Worker* and the extensive collection of the Catholic Worker movement. She offers us an excellent work of scholarship on a small but significant aspect of American Catholicism and American radicalism and a most unusual figure in American journalism.

DAVID J. O'BRIEN
Holy Cross College

SUSAN STOUT BAKER. *Radical Beginnings: Richard Hofstadter and the 1930s.* (Contributions in American History, number 112.) Westport, Conn.: Greenwood. 1985. Pp. xxi, 268. $35.00.

The most surprising biographical documentation in this book is of Richard Hofstadter's entry into the Communist party after the purge trials, in 1938, and exit from it prior to the Nazi-Soviet Pact, in 1939. The most provocative historiographical hypothesis advanced by Susan Stout Baker is that Hofstadter's writings are best understood as a link between the Old Left and the New Left. The large question with which a reader closes this book concerns the influence of Marxism on American historical interpretation.

Leaning on Erik Erikson's theory that the decisive formation of identity occurs between ages eighteen and twenty-five, Baker examines Hofstadter's youth during college and graduate school in most detail. Some biographical attention is paid to his earlier years, but none to the years after thirty. Baker discusses Hofstadter's historical writings throughout his life and thus inevitably tends to read her account of the boy's life into her analysis of the man's scholarship.

Born in Buffalo in 1916 of a Jewish immigrant father from Cracow and a native American Lutheran mother of German descent, Hofstadter was baptized Lutheran and later confirmed Episcopalian. In high school he was a freshman cheerleader; he debated, played tennis and golf, and joined a fraternity that was known to exclude Jews. He was president of his senior class and valedictorian.

Baker's depiction of his absorption into Harvey Swados's family while a student at the University of Buffalo portrays an awakening of Hofstadter's Jewishness. He matched his major in history with one in philosophy and was educated in the Hegelian dialectic. Politically radicalized as an undergraduate, he married at age twenty Swados's sister Felice, an older philosophy graduate, and subsequently undertook abortive studies in law in New York City. Graduate study in history at Columbia with Merle Curti, who is characterized by Baker as a Marxist in the late 1930s, allowed Hofstadter to reconcile academic history with Marxism through the historical analysis of ideas in their social and economic contexts. By age twenty-four he had passed his oral examinations in history and selected Social Darwinism as a doctoral dissertation topic. His later twenties, during World War II, were spent teaching at the University of Maryland. A son was born to the Hofstadters in 1943, and in 1945 Felice died of cancer. That same year *Social Darwinism in American Thought, 1860–1915* was published, and Hofstadter was invited to a position at Columbia.

Baker characterizes Hofstadter's bachelor's thesis on the tariff and homestead issues in the Republican campaign of 1860 and his master's thesis on cotton tenants in the Great Depression, as constrained by "the stultifying aspects of Marxist scientism" (p. 135). But, after Curti liberated Hofstadter by showing the promise of intellectual biography, Hofstadter's literary gift joined his complex sensibility with his talent for analysis. *Social Darwinism* began a series of scholarly triumphs.

Baker devotes the final fifth of the book to a review of portions of *Age of Reform* (1955), *Anti-intellectualism in American Life* (1962), and *The Idea of a Party System* (1969) to show that the "dialectical structure" in these works "indicates how Hofstadter retained and developed the philosophical underpinnings of Marxism even as he eschewed the surface political elements of the theory" (p. 191).

This book can safely be predicted to cause rereadings of Hofstadter's works for confirmation, qualification, or contradiction of Baker's interpretation of Hofstadter as a residual Marxist. The smoldering issue of the influence of Karl Marx, in relation to that of Charles Beard, will be fanned again. And some may ask whether McCarthyism encouraged radical historians to stay "in the closet" during the years following the Second World War.

Finally, in addition to pursuing these questions, students of postwar historiography will want to try to account for Hofstadter's intellectual charisma for scholars who came of age between the late 1940s and the early 1960s. It was not because Hofstadter was perceived as the author of the interpretation

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attributed to him by Baker, for her book presents a Hofstadter most readers have not recognized previously.

ROBERT ALLEN SKOTHEIM
Whitman College

DEBORAH E. LIPSTADT. *Beyond Belief: The American Press and the Coming of the Holocaust, 1933–1945*. New York: Free Press of Macmillan. 1986. Pp. xi, 370. $19.95.

Deborah E. Lipstadt's well-written, thorough, and thought-provoking book analyzes the news and editorial coverage in the American press of the Nazi actions and policies that would eventuate in the Final Solution. Her account shows that, although newspapers and magazines carried a continuous and fairly accurate account of the fate of the Jews, they failed to convey an understanding of the centrality of anti-Semitism to Nazism or to indicate, even after it had become clear, that the Nazi state ultimately sought to murder an entire people. Thus, although reporters described and strongly condemned aspects of the war on the Jews, the stubborn reluctance to believe fully or expose forthrightly the evidence they uncovered diminished the impact of their reports. Moved by a professional skepticism hardened by the atrocity propaganda of World War I, journalists wrote in ways that suggested they doubted the reliability of their own accounts. Even after 1942, when evidence was overwhelming and skepticism had diminished (*Christian Century*'s consistent doubting stands out as a sorry exception), the press treated the Jew as merely another victim of generalized Nazi barbarism. As a result, Lipstadt contends, the press missed an opportunity to generate pressure for serious efforts at rescuing or providing haven to those who eluded Hitler's grasp.

Beyond Belief not only deepens our understanding of both the Holocaust and the American press but is also provocative in ways the author may not have intended. The book's indictment of the press for what it failed to do rests on the dubious implication of a standard for the behavior of the press. The press, the author suggests, should have worked to ensure public understanding of the calamity befalling European Jewry; at the same time, however, her account makes clear that journalists, like many others, were often unsure of what was happening. They did not always command the facts and were confronted with telling not one but many potentially momentous stories. Moreover, governmental policy, which the press generally sustained out of conviction or patriotism, shied from depicting the war in terms that suggested a crusade to save anybody or anything except, perhaps, the "American way." Lipstadt seems to expect journalists to have been contemporary historians. But the press did not have access to the complete record, the detachment from contemporary affairs that permits fearless exploration of the truth, or the perspective that allows historians, including Lipstadt, to conclude that hundreds of scattered bits of information concerning Germany and the Jews constituted a single consciously constructed catastrophe.

Lipstadt's account also suggests throughout that, had the press been more "responsible," the fate of at least some Jews might have been different. The author is on more realistic ground, however, when she acknowledges that quite possibly a different press policy would not have changed either public indifference or official inertia, for neither of them was based on ignorance alone. The seeming inconsistencies between Lipstadt's humane instincts and her historical assessments convey the anguish and frustration we must all feel when confronted with evidence supporting Stalin's dictum that when one man dies it is a tragedy but that when thousands die it is merely a statistic.

Journalists share the insensitivities of their culture, and, although Lipstadt seems to understand this, she refuses to accept it. Instead, she looks to the performance of the liberal press as a model of what might have been. But these journals thrived on the exposure and condemnation of fascist crimes. In general, crusades of the kind Lipstadt wishes for were the life blood of the liberals. And, just as the liberal press reflected its reader's predilections, so did the general press. Unfortunately, most Americans were suspicious of atrocity accounts and were hostile to providing sanctuary to large numbers of foreigners regardless of their plight. Americans were unwilling to believe, and unlikely to welcome, stories that reminded them of moral obligations they had no intention of fulfilling.

This well-researched and thoughtful book provides a moving reminder of a tragic aspect of the human condition.

RICHARD W. STEELE
University of New Hampshire

HAROLD G. VATTER. *The U.S. Economy in World War II*. (Columbia Studies in Business, Government, and Society.) New York: Columbia University Press. 1985. Pp. x, 198. $25.00.

Harold G. Vatter offers his reader a short and useful account of the U.S. economy and domestic economic policy in the era of World War II (1939–47) and beyond. His theme is that World War II accelerated and provided the basis for the mixed economy of today.

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The introductory two chapters are on wartime preparations (1939–41), the first providing basic macroeconomic statistics—unemployment rates, size of real gross private investment, size of real personal consumption, military expenditures as a percent of gross national product, and so forth—and the second chronicling the development of government agencies and the administration of preparedness. Chapter 3 is on farming and small manufacturers (and large ones by comparison). Chapters 4 and 5 turn to wartime administration, and chapter 6 considers U.S. fiscal and monetary policy. Chapter 7 is devoted to social change (from black participation in the labor force to the postwar baby boom) and chapter 8 to other postwar changes and the consequences of the war. The book has footnotes but no bibliography.

The book is like a sandwich: chapter 1 has material on the breakdown of American isolationism and the war in Europe (it does tell of Lend-Lease); chapter 8 deals with the Bretton Woods institutions (the International Monetary Fund and the International Bank for Reconstruction and Development) and the cold war; in between, the book is exclusively concerned with domestic issues. It is even hard to realize that the volume is discussing America's involvement in a world war. Vatter does not explain why America in 1939 was dominated by isolationism nor why the country in 1945 had become a responsible leader (my phrase, not Vatter's) in the world economy. He also makes no mention of the private sector's international economic involvements during the war or of the role of multinational corporations between 1939 and 1945. Nor does he provide international trade, international investment, or international migration statistics to complement the rich and complete domestic data presented. Likewise, although domestic wartime logistics are discussed at length, the more complex international supply problems are shortchanged. Vatter includes little on U.S. foreign economic policies in wartime. The many recent studies on U.S. international oil policies during and immediately after the war are completely neglected, which is a serious omission in a book that aims to document the major changes in U.S. economic policies that emerged out of World War II. Although both world wars are, on occasion, compared, Vatter does not highlight the learning process; he seems to forget how close 1918 and 1939 were (a man who was twenty-two in 1918 was only forty-three in 1939).

By contrast, this slim volume is remarkably complete on domestic matters, although I missed materials on changes in America's financial institutions. Corporate profits as a percent of assets are provided, but prevailing interest rates are not. Vatter devotes only one page to technological developments and does not include probably the most profound development of all—the first computer. In sum, though good on charting domestic macroeconomic trends and social change and helpful on the maze of government agencies and their role in directing domestic production, distribution, and pricing, the book leaves out some extremely important topics. The omissions notwithstanding, many economic historians will find this brief volume valuable as supplementary reading in introductory U.S. economic history courses.

MIRA WILKINS
Florida International University

ALEX ROLAND. *Model Research: The National Advisory Committee for Aeronautics, 1915–1958*. In two volumes. (NASA History Series.) Washington, D.C.: Government Printing Office. 1985. Pp. xx, 391; ix, 393–769.

One-third of the more than thirty titles now available in the NASA History Series are reference works; the other two-thirds are narrative histories of various programs, projects, and institutions. *Model Research* fits both categories. Alex Roland worked for a decade to bring Walter T. Bonney's history of the National Advisory Committee for Aeronautics (NACA) to completion, transforming a public relations portrait of the nation's first research agency for aviation into a critical history. Volume 1 is almost four hundred pages of narrative; volume 2 presents eight appendixes of reference documents.

The author organizes his book in a dozen, roughly symmetrical chapters covering five-year time spans. He begins his narrative about 1910 with a chapter entitled "Quest for a National Aeronautical Laboratory" and ends in 1956–58 with "From NA¢A to NA$A." In between, chapters such as "Tunnel Vision, 1919–1926," and "New Genius, Old Bottle, 1945–50," which features Hugh L. Dryden as director, are neatly constructed to capture the essence of the rise and fall of the NACA's official establishments. From 1915 to 1958 the NACA was "arguably the most important and productive aeronautical research establishment in the world" (preface). The NACA published more than sixteen thousand reports during its forty-three-year lifetime, ranging from studies of later Wright flyers to rocket planes and missiles. Roland avoids what he calls "technical history"; rather, he presents a political and institutional history aimed at three themes: the uniqueness of the institution, its personnel policies, and its instrumentation facilities.

Originating by parliamentary skullduggery and legislative legerdemain in the March 1915 naval appropriations bill, the Advisory Committee for Aeronautics was made "national" by the first official action of the committee in April 1915. In the mid-

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1920s the advisory committee became a research agency responsible for a national laboratory near Norfolk, Virginia. Gradually a West Coast laboratory was seen as necessary to accommodate the aviation and airframe industry located largely in California. Thus arose the Ames Laboratory just south of San Francisco in 1940. That was soon followed by the establishment of the Lewis Laboratory in Cleveland during World War II. The NACA concentrated on aerodynamics and achieved preeminence in cataloging airfoils, in developing optimal cowlings for radial engines, in testing all sorts of equipment, and in solving various aerodynamic problems.

At its peak, the agency employed some eight thousand workers and served primarily the military-industrial complex by trouble-shooting aerodynamical problems. Unlike the Manhattan Project and the Atomic Energy Commission, which were dominated by scientists, the NACA and its successor agency, NASA, have always been dominated by engineers. Roland tells most of his story with biographical vignettes of leading characters, giving special attention to seventeen leaders.

Roland shows great zeal in attacking the NACA's corporate narcissism and bureaucratic struggle for survival. That NACA engineers failed to foresee the importance of jet engines, missiles, and rockets in the post–World War II era eventually led to a loss of confidence in the NACA, especially after the sputniks of 1957. Roland concludes with seven reasons why Congress and the Eisenhower administration subsumed the NACA under NASA in 1958: the NACA was merely a committee; it had grown too large and expensive; it ceased to offer unique services to industry and the military; it was too conservative; it had missed too many opportunities to be credible; it was in collusion with too many representatives of the military-industrial complex; and it had long ago achieved its aeronautical goals.

In his set of appendixes constituting volume 2, Roland has provided a type of "data book" that may serve many purposes. Primary legislation can be traced in a thirty-page appendix. The NACA's subcommittee structure, budget appropriations, and expenditures are set forth. Personnel organization charts, key positions, and discontinued positions are listed. Prime facilities for research, development, and testing are described. The NACA report system is analyzed in appendix G. Finally, about forty-six miscellaneous documents covering almost two hundred pages illustrate other significant events related to the NACA from 1910 to 1958.

Altogether, *Model Research* is a valuable addition to the literature on the prehistory of NASA. Roland's interpretation of the NACA from its own files is so sour that Jimmy Doolittle's foreword has to apologize for the bitter taste. That this was published under NASA's imprimatur is a wonder. But this book is supposed to be about the research and development behind manned flight in the twentieth century. The exhilaration of flying higher, farther, faster, and more safely is, however, sacrificed to a portrayal of bureaucrats, technocrats, and plutocrats whose constant effort to preserve their own power may sometimes seem to cause disasters like that of the *Challenger*.

LOYD SWENSON, JR.
University of Houston

CHARLES JOSEPH GROSS. *Prelude to the Total Force: The Air National Guard, 1943–1969*. (General Histories.) Washington, D.C.: Government Printing Office. 1985. Pp. xv, 251.

This well-illustrated, small book was prepared in 1978–79 as a doctoral dissertation at Ohio State University, and the Office of Air Force History has printed it in its General Histories series. Charles Joseph Gross became an air guardsman in 1972 in the Ohio Air National Guard and served a tour as a targets analysis officer in Vietnam. He is now a civilian historian at Headquarters Air Force Systems Command.

The introduction returns to May 30, 1908, when the First Aero Company of the Signal Corps of the New York National Guard was put together at the Park Avenue Armory. After this, the history is strictly limited to the period 1943–69 and is mostly concerned with high-level policies and actions. Much too often, Gross dismisses the early Air National Guard as a "flying club." He has obviously accepted Secretary of Defense Melvin R. Laird's conception of a "total force" composed of military reserves, regular forces, and civilian employees—a concept that, when promulgated in 1970, also explained the way that the armed services would be able to save money by reducing the number of those on active duty.

In detailing the Air National Guard's collective history since 1943, the author describes in succession the United States Air Force denigration of reserve components after 1945, the laborious reawakening of reservist units for the Korean conflict, an improved mobilization of reservist units during the Berlin crisis of 1961-62, and the occasional employment of reserve units during the war in Southeast Asia. Finally, when the Tet offensive in Vietnam and the Pueblo incident in Korea demanded it, the air guard showed its value by quickly attaining war footing. Other aircrews of the air guard routinely flew augmentation airlift missions to resupply American forces in Southeast Asia. In the supplementation of NATO in 1960–61 mobilization and deployment were generally all that was

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wanted, but the reinforcements would doubtless have found it difficult to operate against an enemy because most were not manned, trained, or equipped immediately to assume full operational and maintenance responsibilities of the bases. As a result, some now fear that planned deployments of U.S. air reservist forces to support NATO in a future conflict could encounter similar difficulties in finding suitable European base support.

In this excellent study Gross provides the first portrayal of policy making in the air guard. One may hope that the Office of Air Force History will use the author's talent to write a genuine "general history" that captures the full record of Air National Guard and Air Force Reserve units through the 1930s and World War II. After all, the old Air National Guard observation squadrons, first organized as state troops in the 1920s and 1930s, ultimately proved to be the genesis of the vital U.S. Army Air Forces tactical air reconnaissance units of World War II.

ROBERT F. FUTRELL
Montgomery, Alabama

GUNTHER GERHARDT. *Das Krisenmanagement der Vereinigten Staaten während der Berliner Blockade (1948–1949): Intentionen, Strategien und Wirkungen.* (Historiche Forschungen, number 25.) Berlin: Duncker and Humblot. 1984. Pp. 366. DM 128.

The analysis of crises, diplomatic bargaining strategies during crises, and crisis-management tactics and strategies has become a topic both for historical case studies and for comparative studies by social scientists. Glenn H. Snyder and Paul Diesing (*Conflict among Nations: Bargaining, Decision Making, and System Structure in International Crises* [1977]), Richard Ned Lebow (*Between Peace and War: The Nature of International Crisis* [1981]), and Hannes Adomeit (*Soviet Risk-Taking and Crisis Behavior: A Theoretical and Empirical Analysis* [1982]) have contributed methodological tools, theoretical concepts, and empirical insights to the analysis of crises. These interdisciplinary efforts—all three books include or specifically focus on the Berlin crisis—and the refreshing methodological debate among West German historians during the 1970s have had no impact on this historical dissertation.

Gunther Gerhardt focuses his traditionalist historical treatment on the role of the U.S. government both in the formulation of policy and on the local level in Berlin during the blockade, the first major crisis in the cold war. The book is divided into four parts. In the first part Gerhardt describes the specific reactions and measures the illegal Soviet blockade of Berlin provoked from General Lucius Clay and his foreign policy adviser, Robert Murphy, the Office of Military Government for Germany, United States (OMGUS) representatives in Germany, and the impact of their suggestions for "Operation Vittles" on the decision of the political leadership in Washington not to retreat from but to strengthen the U.S. presence in Berlin. In the second part the author describes in detail the role of different institutions in the decision-making process during the Berlin crisis. Gerhardt discusses the OMGUS, the State and Defense Departments, the Joint Chiefs of Staff, President Truman, the National Security Council, the U.S. Congress, the Central Intelligence Agency, and nongovernmental social actors such as the Council of Relief Agencies Licensed for Occupation in Germany (CRALOG) and Cooperative for American Remittances to Europe (CARE). The focus of the third part is on the complementary measures undertaken by the U.S. government to integrate the politicians and the people of Berlin into the management of the crisis through the active use of propagaganda and public opinion polls but also through close cooperation with the mayor of West Berlin, Ernst Reuter, as in the creation of the Free University. The fourth deals with the implications of the management of the crisis for the political and legal position of Berlin, for the reaction of the government and the public of the United States to the demonstration that firmness and resolve could uphold Western interests without the use of force, and for the relationship between the American occupying forces and the citizens of Berlin. In an appendix Gerhardt provides an extensive list of available historical sources and studies on the Cold War, the German question, and the Berlin problem, as well as twenty-five major documents reproduced as facsimiles and in German translation and personal and subject indexes.

Although Gerhardt's book contributes many new details and insights to the understanding of the first Berlin crisis, it nevertheless has some major shortcomings. The broader context and controversial historiography of the evolving Cold War and the role of the Berlin blockade in the presidential election of 1948 should have been given a broader emphasis. Instead of referring at length in the introduction to the obvious, Gerhardt should have briefly analyzed studies of the Berlin crisis by social scientists, and the impact of this crisis on bilateral crisis-management and crisis-prevention strategies since World War II.

HANS GÜNTER BRAUCH
University of Stuttgart

WILLIAM H. CHAFE. *The Unfinished Journey: America since World War II.* New York: Oxford University Press. 1986. Pp. xii, 516. $24.95.

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Having given us *Civilities and Civil Rights* (1980), that remarkable study of Greensboro's civil rights movement, as well as several fine books on women in American history, William H. Chafe now undertakes an interpretive summary of the forty years since the Second World War. This recent book is especially welcome because, as he points out in his preface, "most of us know less about the decades that have shaped our immediate lives than we do about the era of the American Revolution" (p. vii).

Chafe's new book fills this need with extraordinary skill. It will be ideal as a textbook for the postwar years and can also serve as a reminder to the rest of us of events dimly remembered or totally forgotten. The narrative, rich and crowded as it is, is given coherence by a strong conceptual framework and made enjoyable by some fascinating personality sketches and vivid anecdotal tidbits.

The framework emphasizes race, class, and gender. Chafe examines "progress" in America through the sensibilities of those most neglected in traditional public policy and orthodox history: blacks, women, and the poor. Such a viewpoint leads Chafe to look beneath the surface of national self-congratulation and to inspect critically the policies followed since World War II by liberal and conservative presidents alike.

Although Chafe begins with Roosevelt and the war years, lingers thoughtfully on Truman, Eisenhower, and the cold war, and takes us right up to the reelection of Reagan in 1984, the heart of the narrative is the 1960s of Kennedy and Johnson. Here Chafe is at his best. An admirer of the New Deal–Great Society reforms on behalf of the poor, he nevertheless understands the limits of those measures because he insists on observing them through the eyes of the underclasses.

Thus, to blacks engaged in the struggles for equal rights in the tumultuous years following the Montgomery bus boycott of 1955, Kennedy's "liberalism" was very much a sham. Politically an opportunist, the president was reluctant to change, and only when blacks marched in the streets by the tens of thousands, went to jail, and deeply embarrassed the country, which had so long boasted to the world of its democracy, did he begin to act. Johnson's "war on poverty," reaching its climax in a burst of legislation in 1965, was from the standpoint of the poor a skirmish rather than a war, bringing help to many but leaving the structural causes of poverty untouched and blacks and women still far behind.

Most important, liberal reform was fatally flawed because it was joined to an aggressive and costly foreign policy, of which the Vietnam war was only the most ugly and most protracted manifestation. Based on a fanatical and simplistic anticommunism, this policy not only supported right-wing dictatorships abroad but also limited severely the possibilities for social and economic change at home. Civil rights and antipoverty activists began to question the capacity of the American system to solve its deepest problems.

Despite dramatic cultural changes, especially in race and sex relations, economic uncertainty and a foreign policy still rigidly based on anticommunism continued through the Nixon and Carter years and into the Reagan presidency. One striking postwar phenomenon was the apparent loss of faith in the electoral process, with half of the voting population abstaining from presidential elections.

Chafe concludes, however, not with cynicism but with a call for Americans to keep in mind the surprising changes of the 1960s and 1970s and to act, even in the smallest of ways, for a more just society.

HOWARD ZINN
Boston University

SHERI I. DAVID. *With Dignity: The Search for Medicare and Medicaid*. (Contributions in Political Science, number 132.) Westport, Conn.: Greenwood. 1985. Pp. xiv, 194. $27.95.

The passage of the Medicare Act in 1965 marked a turning point in the provision of medical care for the poor and elderly. After decades of debate about the merits of national health insurance, Congress finally passed a compromise bill that fell far short of the goals of "liberals and labor" but went considerably beyond the wishes of organized medicine and conservative lawmakers. In this book Sheri I. David briefly traces the history of this landmark legislation. Eschewing both historical and historiographical context, she begins her account abruptly in 1957 with Representative Aime Forand's proposal to provide hospital, surgical, and nursing-home benefits to recipients of social security. Basing her narrative largely on congressional hearings, interviews with key participants, and newspaper accounts, particularly from the *New York Times*, she then proceeds bill by bill to describe the fate of such legislation.

If the book has a thesis, it concerns the pivotal roles played by two heroic figures: Wilbur Cohen, a social security expert who became assistant secretary of the Department of Health, Education, and Welfare during the Kennedy administration, and Representative Wilbur Mills, a Democrat from Arkansas, who assumed the chairmanship of the House Ways and Means Committee in 1957. David describes the brilliant and indefatigable Cohen as a master of "the political game of gradualism or incrementalism" (p. 39), who was willing to accept one slice of salami at a time but never lost sight of the whole sausage. She portrays Mills as a political genius whose prescient concern about runaway costs

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at first prevented the passage of any Medicare-type legislation but whose successful bill in 1965 represented "a master stoke of political compromise" (p. xiii). "Of the 535 members of Congress," claims David, "Mills was the only man who thoroughly understood the Social Security System and the complicated actuarial basis of making estimates of financing needs for the future" (pp. 14–15).

David expresses the hope that, "by recalling the history of Medicare, we can . . . examine present and future possibilities more intelligently" (p. xiv). Unfortunately, her book contributes only modestly to this effort.

RONALD L. NUMBERS
University of Wisconsin

DAVID ZAREFSKY. *President Johnson's War on Poverty: Rhetoric and History*. University: University of Alabama Press. 1986. Pp. xxiii, 275. $24.50.

The War on Poverty was not only one of the most ambitious reform projects of post–New Deal liberalism but one of its most grandiose rhetorical ventures as well. David Zarefsky views the rhetoric of the project as the key to its fate; he "locates the problems of the War on Poverty in its *discourse*" (p. xi). His concentration on the struggles fought over the symbols of the antipoverty effort makes for an engrossing analysis of the War on Poverty from a fresh angle of vision.

Zarefsky shows that the rhetoric of war helped facilitate the swift passage of antipoverty legislation in 1964 but created symbolic dilemmas that undermined the program once it began to be implemented. War as a metaphor gave the Johnson administration a means to promote an antipoverty program as an urgent national undertaking. The metaphor "captured presumption and thereby reduced the administration's burden of proof in appealing for recruits" (p. 36). It served as a device to overcome partisan overtones in an election year and to inspire a public with little initial awareness of the issue.

But the exaggerations and the ambiguities in the original rhetoric of the War on Poverty were to haunt the program after its legislative passage. Because results fell far short of announced objectives, the Johnson administration tended to inflate limited accomplishments and stress future gains. These patently rhetorical stratagems fueled attacks on the administration's credibility and charges that its hyperbole would only further the disillusionment of the poor.

The weapons and tactics established in the original definition of the War on Poverty proved to be as problematic as the military objective. The principal tactic, "community action," was ambiguous enough to win adherence from different political camps at the outset. Once community action came to be identified with the idea of political organization of the poor, however, the resulting controversy carried heavy political costs for the Johnson administration. This controversy overshadowed the successes of many community action projects, and the public identification of community action with political confrontation led by militant black activists meant that the Johnson administration was saddled with an untenable political image.

Zarefsky's book provides more than a cogent analysis of the War on Poverty. It is a valuable study of rhetoric and history, focusing our attention on how symbols define political situations and how they are modified over time in the face of new events and pressures. The author's account of the Johnson administration's rhetorical moves and modifications in the face of defeats in the "unconditional war" declared in 1964 is particularly enlightening.

Although Zarefsky makes a strong case for the importance of rhetoric, he does overstate its causal force. The original rhetoric of the War on Poverty was not as thoroughly martial as he suggests; "community action" was hardly a military metaphor, and the "emphasis on cost-consciousness" directly contradicted the imagery of total mobilization (p. 50). More important, the conflicts that developed during implementation might well have happened even had the rhetoric been less grandiose and ambiguous. The struggle between organizers of the poor and big city mayors, for instance, was a battle over power more than over an ambiguous concept. In attributing so much of the War on Poverty's later troubles to its original rhetoric, Zarefsky also pays insufficient attention to the changing political climate. By 1966 a growing white backlash was reducing congressional support for all of Johnson's domestic agenda; if the mounting criticism of the War on Poverty in part reflected its special flaws, such criticism was not unique to this policy. As Zarefsky himself recognizes in his concluding chapter, the symbolic problems of liberal reform revealed in the War on Poverty were connected to deeper ideological and political problems that have bedeviled contemporary American liberalism.

BRUCE MIROFF
State University of New York, Albany

CANADA

BRUCE G. TRIGGER. *Natives and Newcomers: Canada's "Heroic Age" Reconsidered*. Kingston, Ont.: McGill-Queen's University Press. 1985. Pp. xiii, 430. $35.00.

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This latest volume by Bruce G. Trigger attempts to reassess the beginnings of European colonization in the St. Lawrence Valley in order to "make the early history of Canada less a chronicle of the pursuits of a chosen few and more the study of the activities of all native Canadians and European newcomers" (p. 343).

The opening two chapters give an informative survey of trends in historical and anthropological scholarship since the nineteenth century and will undoubtedly contribute to making members of both disciplines more aware of each other's work. Indeed, these chapters help underline one of the principal themes of this volume: that more archaeological research is required to free both historians and anthropologists from much of the gratuitous revisionism that has been prevalent in the recent literature. Trigger then devotes his attention to the sixteenth century, incorporating recent archaeological work to draw a well-balanced picture of the impact of early European activities on the native populations of the Northeast. The following chapter, covering the period 1600–32, is characterized as one of "nondirected change" (p. 224), when Europeans profited from the fur trade but did not have the manpower or the knowledge to control, let alone radically alter, native ways of doing things. This changed after 1632, because the fur trade created new dependencies and the religious activities of the Jesuits dramatically affected tribes such as the Huron and created internal turmoil. The concluding chapter is a plea for more research to be done on ordinary people (fishermen, fur traders, and natives), who have been hidden behind famous figures such as Samuel de Champlain, but who are the true founders of Canada.

Specialists of this period will find little that is totally new in this book. Perhaps the most significant departure from Trigger's earlier work is his revised interpretation of the sixteenth century, which places much less emphasis on the fur trade as a catalyst for some of the dramatic changes that took place, notably, the disappearance of the St. Lawrence Iroquoians. They will find some nit to pick here and there—for example, the statement that huge profits were made in the early seventeenth century (p. 188), which is based on an incomplete analysis of the total costs involved in outfitting transatlantic maritime ventures—but the scholarship is basically sound and the conclusions generally justifiable.

This book is not, however, intended to be read mainly by specialists but is aimed at a much wider public. In this respect it is a very useful contribution to early Canadian history. Students and the general public will find a coherent tale of Canada's early development that finally gives native peoples their rightful place. It also should help bridge the gap that still unfortunately exists between historians and anthropologists.

JOHN A. DICKINSON
Université de Montréal

ALLAN GREER. *Peasant, Lord, and Merchant: Rural Society in Three Quebec Parishes, 1740–1840.* Toronto: University of Toronto Press. 1985. Pp. xvi, 304. Cloth $35.00, paper $15.00.

In 1840 a British immigrant compared the "superiority of farming" in English-speaking Upper Canada with cultivation in the adjacent French-speaking province. "The lower Canadians or 'Habitants,'" he wrote, were "probably the worst farmers in the world, and what is most singular, they will not improve." Allan Greer sets out to refute this sort of "bad press," and he does it with passion, ingenuity, and force. He argues that rural society in Lower Canada was "feudal" in the sense that the farmers were primarily concerned with producing for their own needs and that whatever surplus remained was largely taken up by aristocratic and ecclesiastical "exactions." The merchants who appeared in the area in the late eighteenth century grafted themselves onto the exploitative superstructure without altering the socioeconomic order. It is well known that farms in pre-1760 French Canada were social units focused on family welfare, not enterprises for commercial production. What is original in Greer's argument is the claim that production for family needs remained the farmers' goal after 1760 and the denial (contrary to Jean-Pierre Wallot and others) that a commercial revolution occurred in agriculture before 1840. The ideal of family self-reliance explains the *habitants'* indifference to purely commercial crops and scientific agriculture.

The subtitle suggests a microhistory, yet the writer's impressive knowledge of current literature allows him to make comparisons with England, France, and New England. The people of the three parishes of Sorel, St. Ours, and St. Denis participated in the uprisings of 1837–38. Surprisingly, the author does not explicitly link the situation of the farming folk with their support for rebellion. Greer rejects the W. H. Parker–Fernand Ouellet thesis that an agricultural crisis was brought on by rural overpopulation, excessive subdivision of farms, and soil exhaustion (although crop yields are said to be impossible to establish). One is left to assume that the political grievances of the 1830s resulted in some way from "domination" and "exploitation" by the seigneurs and from crop devastation by the wheat midge, but the connection is not made clear. This failure to link the observations about rural life with the major military and political events of the region is the book's greatest defect.

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Apart from the *habitant*-farmers, other social groups are given cursory and unsympathetic treatment. The country craftsmen are mentioned, but their social and economic position in rural society is undefined (pp. 196–97). With the exception of one local German Jewish merchant whose papers were studied, traders are caricatured as "parasitic intermediaries between productive systems" whose principal care was "buying cheap and selling dear" (p. 175). Seigneurs and priests are shadowy figures who also squeeze as much as they can from the farmer. One seigneur is likened to a "robber baron," and his father is said to have made money by "preying on the Indians and fur traders" (pp. 107–08). The writer's partisanship is equally apparent when he places the seigneur's mill toll at 1/13 instead of 1/14 (p. 136) and when he accuses the seigneurs of stifling industrial entrepreneurship among farmers (p. 133). Likewise, it seems excessive to charge British merchants with entrapping *Canadien* consumers with rum (p. 159); the *habitants*' fondness for brandy predated the British conquest. This picture of class conflict and oppression is offset by the writer's admission that the rural folk used evasion, procrastination, and shrewd bargaining to reduce the "exactions" of seigneurs, merchants, and priests.

When the censorious Marxist view of social relationships is laid aside, the book is imaginative, persuasive, and sometimes brilliant. This is the same with chapter 3, "Generations of Peasants." Greer is particularly adept at threading his way through the complex interplay of French civil law and actual usages. The skillful portrayal of material life and changing practices among the peasantry is in the tradition of R. C. Harris, Ouellet, and Louise Dechêne. The originality of his use of certain types of evidence, such as the geographic distribution of family surnames, is admirable. In a tour de force, Greer uses property deeds to show how a postmortem egalitarian division of estates among children, in consultation with the entire family, gradually gave way to a parental favoritism toward children who promised their retired parents a comfortable and secure old age. This practice and the reduction in available land added to the landless laborers in each parish. It is this ingenious disclosure of peasant mores that makes one regret that the author did not attempt to reconstruct the social and political ideology of the *habitants*. He chose not to consult the court records, which would have revealed popular beliefs and values. One is left with the impression that economic survival strategies dominated the thoughts of the peasantry.

The difference between Sorel Parish, whose smallholders survived with a supplementary income from work in the fur trade, and nearby St. Denis, a grain-exporting parish with large farms, is a warning against facile generalizations about rural life in French Canada. This book is not a complete or detached view of rural life; it is a passionate and intelligent brief in defense of the *habitant*-farmer and an indictment of his "exploiters."

PETER N. MOOGK
University of British Columbia

JACQUELINE PETERSON and JENNIFER S. H. BROWN, editors. *The New Peoples: Being and Becoming Métis in North America.* (Manitoba Studies in Native History, number 1.) Foreword by MARCEL GIRAUD. Lincoln: University of Nebraska Press. 1985. Pp. xvi, 266. $22.50.

This collection of conference papers has as its objective a comprehensive account of Métis origins, ethnic consciousness, and struggle for recognition. In spite of the claim on the dust jacket, the book concerns race relations as much as ethnogenesis and will long remain an indispensable introduction to the complexity of Métis communities in North America.

In a brief foreword Marcel Giraud, who wrote the first major work on the Métis, traces the development of his scholarly interest in the subject. The opening essay by Olive Dickason leads the reader to expect more than is achieved in the first three essays dealing with origins. Dickason makes tantalizing hints, for example, regarding the origin and use of the term "Métis," contemporary views on intermarriage, and the evolution of *métissage* in the Americas, but this intellectual and sociocultural context could have been developed to greater advantage.

The mixed-blood populations of the Maritimes and St. Lawrence Valley simply disappear from the book after the first essay, as they do from most historical records after early contact. Those of the Great Lakes basin fare much better at the hands of Jacqueline Peterson, because in this region distinctive households, and then communities, developed from the small number of individuals near the French posts and Catholic missions. As the fur trade and settlement pushed farther westward, so did the Métis. A pattern of migration, reaching to Montana and northern Alberta, emerges from the essays, as does a clear sense of the evolution of Métis societies. The characteristics of early communities were that "wealth was fairly evenly distributed, status distinctions were few" (p. 41). In later communities along the Red River two divisions stood out: first, between the well-educated elite and "the masses of unlettered, unpropertied natives of the country" and, second, "between the professional farmer and the hunter and plains trader" (p. 112). Verne Dusenberry's article on the landless Indians of Montana reveals the strong influence of the French "fathers"

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and the adaptation of a European hierarchical and authoritarian military organization to the buffalo hunt among otherwise "democratic" people (p. 122).

If the book has a bias, it is the tendency to emphasize the creative dynamism of native societies at the expense of external forces. Peterson, for example, adopts what seems to be a modified Turnerian interpretation according to which Métis culture was largely the product of the upper Great Lakes environment and the fur trade. Lyle Stone's archaeological emphasis on local adaptations buttresses her argument. The European side of the cultural equation cannot, however, be ignored. Canadian culture was a variant of French culture, and Louise Kellogg's interpretation that upper country culture was a selective continuation of Canadian Laurentian culture still has merit. The fur trade was also in some respects a metropolitan enterprise. Stressing the frontier and discounting metropolitan influences lead to the conclusion that the inability of the crown to control the fur trade resulted from local factors in the hinterland. Controls failed not only because of the conditions in the Great Lakes basin but also because of the needs and interests of the Laurentian colony and old France. Dickason and Peterson mention the role of certain royal officials and the military officer corps but fail to mention the effects of changing demands in the European market, the glut of furs by 1696, changing transportation arrangements, and new financial arrangements for the trade. Wood rovers, like brandy traffickers, were merely intermediaries in an economic chain linking native hunters and their womenfolk to Parisian hatters and Leipzig buyers. From emphasis on the European dominance the pendulum now seems to have swung to a fascination with native dynamism in the historiographical stream of Turnerian frontierism and the Wisconsin school of geographers.

The essays touch a number of historiographical bases. Giraud's classic definition, initially using biological terms and then shifting to cultural concepts of community, is updated by Fredrik Barth's analysis of ethnic identity and ethnic boundaries. The question of "nationalism" was identified by Giraud as deriving from the Red River struggle between the Nor'West Company and Hudson's Bay Company, but Peterson supports Joe Sawchuk's view that group identity had long existed and was merely reformulated in political terms early in the nineteenth century. Dickason decries the "reluctance on the part of Canadian historians to acknowledge mingling of races in the Northeast" (p. 20) but proceeds to cite no fewer than four well-known authors who deal specifically with the issue in the context of colonial history. Fritz Pannekoek's thesis that the Manitoba resistance movement of 1869–70 was caused in part "by a sectarian and racial conflict with roots deep in Red River's past" (p. 95) is effectively demolished by Irene Spry, who identifies the divisions at Red River as not between Catholic Métis and Anglican mixed-bloods but rather between social class and occupational status. In so doing, Spry joins Jennifer S. H. Brown in coming close to supporting the thesis of George F. G. Stanley that the struggle was between a primitive traditional way of life linked to the environment and subsistence and an intrusive agricultural way of life.

John Long explores the history of the relatively little known northeastern Ontario Métis and touches a number of historiographical debates. He rejects Charles Bishop's view of the dependency of the Home Guard Cree on the Hudson's Bay Company's largesse in favor of Toby Morantz's view of the dependency of the Europeans on the Home Guard hunters. Trudy Nicks and Kenneth Morgan add a different dimension by showing that Iroquois freemen and Cree adopted a Métis identity in western Alberta to maintain their distinctiveness from Indian social groups. Several papers deal with the context of an emerging identity. Ted Braser's article on the appearance of distinctive art forms, combining Indian manufactures and designs with the French heritage, argues for a cultural flowering in the early nineteenth century. John Crawford's introduction to Métis linguistics does little to clarify the thesis of ethnogenesis; in fact, Michif seems to be spoken also in areas where an ethnic consciousness has not developed.

It is perhaps remarkable that the Métis of the Maritimes and Quebec are not included in the seven cultural regions of the sociographic "map" proposed in the afterword by Robert Thomas. The French component is not represented formally either by a paper on relations between French-Canadians and Métis or on the Métis communities of eastern Canada. The conference was conceived, it would appear, in terms of western Canadian and American perceptions rather than truly North American perspectives as announced in the title.

In sum, my criticisms are few and my praise abundant for a carefully edited, generally well written, and handsomely produced book with pertinent maps, illustrations, and index. Seldom have conference proceedings resulted in a book in which a unifying theme is so well maintained.

CORNELIUS J. JAENEN
University of Ottawa

JASON H. SILVERMAN. *Unwelcome Guests: Canada West's Response to American Fugitive Slaves, 1800–1865*. (Series in Comparative Studies in Race and Ethnicity.)

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Millwood, N.Y.: Associated Faculty Press. 1985. Pp. viii, 215. $22.95.

Generations of Canadians have thrilled to stories of the Underground Railroad, those romantic tales of escape and pursuit that described the flight from American bondage to freedom at the Canadian border. The notion that Canada was thus morally distinguished from the United States became a part of the national myth of racial enlightenment, reinforced by the brutalities accompanying the civil rights struggles of the 1950s and 1960s, which seemed to demonstrate that Americans still had not caught up with Canadians. Unfortunately, the thrilling adventures always ended at the border, with the grateful slaves praising God and Queen Victoria. The assumption was that they went on to lives of freedom and equality in their "promised land," that Canada therefore had no "racial problem" to confront.

In the past fifteen years a substantial literature has arisen to challenge those comfortable assumptions. That the black immigrants suffered from social ostracism, economic disadvantage, and institutional segregation has been increasingly well documented. Nineteenth-century Canadians generally shared the more common racial attitudes of their American neighbors, and, though constrained by English law from the most overt practices, they indulged in personally discriminatory acts and harbored prejudicial ideologies comparable to those of the northern United States. No grounds exist for moral superiority; on the contrary, it is becoming recognized that the old myths have been a barrier to any realistic assessment of the situation of blacks in Canada.

Jason H. Silverman has produced a convenient and readable account of Upper Canadian racism between 1800 and 1865. His conclusions are consistent with the current state of knowledge. Silverman leaves untouched some issues, raised in the recent literature, that might have furthered the comparative objective announced in the preface. Several statistical analyses, for example, have shown that Canada did offer measurable advantages to the fugitives. Property ownership, wages, and the range of available employment were greater north of the border, and to examine the reasons for this might contribute to an understanding of comparative race relations. Despite a sympathetic description of black churches, schools, and newspapers, this book displays an apparent willingness to accept white observers' accounts of black defects and to explain the growth of prejudice as a reaction to the mistakes and failures of the blacks themselves. Such representations were symptomatic of prevailing white attitudes, not of black behavior, and they cannot explain the origins of Canadian racism. Much more comparative work remains to be done before those origins can be traced.

JAMES W. ST. G. WALKER
University of Waterloo

RAMSAY COOK. *The Regenerators: Social Criticism in Late Victorian English Canada.* Toronto: University of Toronto Press. 1985. Pp. x, 291. Cloth $32.50, paper $15.95.

"Scepticism is becoming more general and is protean in its adaptability to circumstance," wrote an anxious Protestant minister in 1880 (p. 41). His anxiety dramatized a "crisis of belief" that beset Canadian Christianity in the last decades of the nineteenth century. The modernism that had also shaken Victorian faith in Britain and the United States had two sources. The Darwinian revolution and historical criticism, each a powerful intellectual force, caused Canadians to question their Christianity, a process that was the more disconcerting because religion had been one of the foundations of nineteenth-century optimism. In this fine book Ramsay Cook examines the disintegration of Victorian complacency and elaborates the implications of the process for the emergence of modern Canadian society.

The response of Canadian Protestantism to modernism was not monolithic. Sir William Dawson elaborated a learned critique of Darwin, and J. W. Bengough satirized historical criticism as self-indulgent pedantry. An ultimately more influential body of opinion in the churches determined, however, that the scientific and historical challenges to Christianity must be confronted in a direct and pragmatic fashion. "The best apology for our faith," the Methodist Conference of 1892 insisted, "is not the scholarly treatise by the learned professor, but the consistent life which mirrors forth the life of Christ" (p. 229). The liberals believed that Christianity would be revitalized as an effective, modern faith "by translating Christianity into a message of social reform and good citizenship" (p. 230). Cook argues with impeccable logic that this substitution of sociology for theology resulted in the secularization of society. His theme is that the crusade of the Christian reformers produced not the kingdom of God on earth but, ironically, the secular city.

Anyone who has read late Victorian reform journals must be struck by the plethora of causes advocated in their pages. Despite the range and complexity of the panaceas, all reformers, except stringent materialists, appear to have been united by a conviction that modern society could be redeemed. Cook observes that reformers frequently used the term "regeneration" to describe their crusades, and the organic metaphor of rebirth is a constant in the

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book. By elaborating their optimism, Cook integrates the causes of late nineteenth-century reformers within a framework of Christian faith.

Such integration is a useful construct, because individual regenerators were as varied and complex as their causes. Cook's taxonomy provides new insights into the thought and work of a dozen more or less prominent late nineteenth-century reformers. Goldwin Smith's search for order is better understood through an implicit comparison with the work of Phillips Thompson; similarly, Thompson's liberal reformism takes on a new dimension when juxtaposed to the causes of the Sage of the Grange. Cook has explored the sources of the regenerators' thought through exhaustive research. The result is not only a better understanding of its content but also a greater appreciation of its range. Indeed, one of the praiseworthy characteristics of this book is its sensitivity. Some of the regenerators were, in their thought, extreme, if not eccentric. Yet Cook never indulges in ridicule nor resorts to easy comic relief. Rather, throughout the book he manifests both sympathy and respect for his subjects; in this regard, Cook's own background as a son of the manse asserts itself in an admirable way.

MacKenzie King, notorious eccentric, linked the world of the regenerators with modern Canadian society. In what is probably the book's best chapter Cook elaborates King's intellectual and spiritual growth from his undergraduate days to the publication of *Industry and Humanity* (1918). Cook argues persuasively that King turned to political economy as the best vehicle by which his faith could be applied to modern society. King was the very epitome of the regenerator who contributed to the secularization of twentieth-century Canada. Cook describes *Industry and Humanity* as a "sermon preached by a socially conscious, but certainly not radical, liberal Presbyterian" (p. 208); it is ironic that an essentially religious tract became the blueprint for a social security system that would make the church obsolete.

That such an important problem has, until now, been insufficiently explored is remarkable. Cook's first-rate book fills the gap, and to the extent that any book can be described as definitive, this one is.

A. ROSS MCCORMACK
University of Winnipeg

RONALD RUDIN. *Banking en français: The French Banks of Quebec, 1835–1935.* (Social History of Canada, number 38.) Toronto: University of Toronto Press. 1985. Pp. xiv, 188. Cloth $20.00, paper $9.95.

Canadian banking historiography is woefully weak. Beyond a surfeit of generally uncritical multivolume commissioned histories, the literature is limited to several useful summaries of the financial sector, a sprinkling of specialized scholarly articles, and a few unpublished theses. In part, this neglect reflects the lack of adequate archival material. Given its long history of mergers and centralization, Canadian banking has relatively few current corporate owners, and they have often destroyed, pruned, or simply failed to make available records under their control. In part, too, this lacuna reflects the underdevelopment of Canadian business history. In an era and discipline where subspecialties proliferate, it is rather remarkable that Canada boasts no business or even economic history journal, is home to no business history associations, and has held only one conference specifically by and for business historians.

If our understanding of general Canadian banking history is weak, our knowledge of business and banking *en français* in Quebec is virtually nonexistent. Ronald Rudin's book is the first scholarly study of French-Canadian activity in a complete sector of the economy. In the absence of such investigation, long-held views of French speakers as disinterested in business, poor risk takers, and, because of some vaguely defined cultural traits, ill suited for capitalist endeavor, have dominated popular and much scholarly thinking.

It is against this background that Rudin's contribution must be put. His succinct study of the general activity of the nine French banks in operation between 1835 and 1925, based as it is on a careful use of published government statistical series, census data, and the holdings of private bank archives, demonstrates that the smaller French-Canadian banks operated in ways virtually indistinguishable from their larger English-Canadian counterparts. The occupational background of directors, the banks' profitability, and the institutions' general structure of assets and liabilities had more to do with size than language of operation. Presumably, too, the incidence of managerial incompetence and dishonesty uncovered by Rudin was paralleled in the English-Canadian sector, although Rudin is quiet on this point.

The operation of French- and English-Canadian banks differed in only one key area: French-Canadian banks concentrated on the French-speaking market and English-Canadian banks focused on the English-speaking sector for the collection of savings deposits and the selling of shares. This market delineation functioned as a two-edged sword. It provided the small French banks a "solid clientele" with which to resist the predatory activities of Canada's large English banks at the turn of the century. Such market specialization, however, also restricted the French banks to

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being forever a minor factor within the general Canadian banking sector.

By viewing the French-Canadian banks as dependent variables reflecting economic and social change within Quebec, Rudin sees them as symbols of the marginal nature of French-Canadian economic activity within Canada. His conclusion that market considerations underlay this marginality is a useful, empirically based corrective to much literature that has focused, often in a discursive and unsystematic manner, on *mentalité* as the key to understanding French-Canadian economic activity. His book, then, is important both for its contribution to Canadian banking history and for helping to lay to rest some shibboleths surrounding the nature of French-Canadian business behavior.

PETER A. BASKERVILLE
University of Victoria

LATIN AMERICA

KENNETH F. KIPLE. *The Caribbean Slave: A Biological History*. (Studies in Environment and History.) New York: Cambridge University Press. 1984. Pp. xiii, 274. $32.50.

In this second of three projected volumes on the biological history of the black in the Americas, Kenneth F. Kiple examines and interprets the health of the Caribbean slave from the sixteenth to the late nineteenth century, focusing on diet, disease, and demography. He relates a story of biological devastation. The 4.5 million blacks wrenched from their West African homeland and enslaved on the islands of the Caribbean, Kiple argues, were the hapless victims of two tragic biological odysseys. The first occurred in their native West Africa. Here age-old experience with rampant disease and poor diet had a natural winnowing effect on the area's peoples. Infants and children were the principal victims. The survivors of this ordeal, Kiple contends, were genetically and biochemically molded—by developing natural resistances to the most deadly forms of malaria and yellow fever in particular—in ways that made them ideal subjects for West Indian slavery after the decimation of the region's Indian populations by European pathogens. The second odyssey began with enslavement in West Africa, encompassed the notorious middle passage, and culminated in a life of servitude on the West Indian plantations. The results were the same: disease and death. Kiple holds that, as before, the chief reasons were a disease-ridden environment and poor nutrition. Moreover, infants and children continued to bear the brunt of the biological onslaught.

Throughout this study Kiple stresses the important relationship between diet and health. He questions, for example, the longstanding view that the reproductive rate of West Indian slaves, which was lower than replacement, was the product of low fertility. In his opinion this phenomenon is attributable to a high rate of infant and child mortality ensuing from poor nutrition.

The black, Kiple points out in a concluding chapter, not only survived West Indian slavery but, owing to improvements in medical care and diet, has even come to dominate the Caribbean demographically. But this development, he warns, has raised the specter of a third disastrous biological odyssey: no longer crucial to the West Indies, the black is, in many quarters, no longer wanted, a situation conducive to another cycle of poor nutrition, deteriorating health, and high infant and child mortality rates.

This work is exhaustively researched, meticulously documented, carefully crafted, and highly readable. Like its well-received predecessor, *Another Dimension to the Black Diaspora: Diet, Disease, and Racism* (1981), it brings research in the biological and social sciences to bear on black history. In the absence of contemporary health records for blacks, Kiple uses modern medical and nutritional studies as a vehicle for reconstructing the biological history of the Caribbean slave. This approach, as he is well aware, leaves him open to a charge of conjecture. But in my opinion Kiple builds a compelling case for his methodology. Indeed, he has produced an important work of enduring value and wide appeal.

JAMES O. BREEDEN
Southern Methodist University

SILVIO ZAVALA. *El servicio personal de los indios en la Nueva España*. Volume 1, *1521–1550*. Mexico City: El Colegio de México. 1984. Pp. 668.

This volume is the first in a projected series covering the entire colonial period. The three volumes on the sixteenth century have already been published, and "if it is possible, others will appear on the seventeenth and eighteenth centuries, and on the beginning of the nineteenth" (p. 14).

Modern scholarly readers will find this and the subsequent volumes on the sixteenth century to be a mine of information on Indian labor. The expository method, however, is old-fashioned and somewhat difficult for students to comprehend.

Silvio Zavala divides the first volume into two parts. In part 1, "Services for the Colonists," he offers a general, legal, and institutional background, followed by sections on labor in agriculture and stock raising, transportation, artisanship, domestic services, urban services such as construction, and supply of necessities such as wood, hay, and charcoal. Throughout part 1 he offers additional infor-

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mation on money, wages, and prices and ends with a look at the peripheral provinces of Mexico, where the viceregal government's main device for enforcement of labor laws in this early period appears to have been the *visita*, or tour of inspection.

Part 2, "Special Services," discusses the Marquesado del Valle (the Cortés estate); services to officials and officialdom; work for the church (including the construction of ecclesiastical buildings); public works; and labor performed for Indian *caciques*, *principales*, and villages. Thus, the overall framework is topical, and within each subsection the organization is roughly chronological. The volume ends with five lengthy documentary appendixes and an extensive bibliography.

The difficulty for the modern scholar—who should persevere nonetheless—is in the manner of exposition. Zavala's book is essentially a sophisticated running commentary on sequentially arranged primary manuscript sources, often extracted or somewhat abbreviated. In the text and footnotes Zavala also engages in what amounts to a bibliographical essay on, and sometimes a dialogue with, the literature published on the particular topic being discussed. The result is a rambling, learned textual exegesis, which is sometimes repetitive but always pointed. By thus sticking so close to his sources, Zavala could perhaps be accused—and, in fact, nearly admits to it (p. 14)—of lacking methodological or theoretical elaboration. His answer is that the reader should consult the more specific monographic works that he cites on each topic.

Scholars would also do well to read this and subsequent volumes with others of Zavala's works to hand. For a general survey of colonial labor, as a companion to this volume, the introductions to the eight-volume *Fuentes para la historia del trabajo en Nueva España* (1939–45), edited by Silvio Zavala and María Castelo are still of enormous help. For comparative purposes, Zavala has published a shorter, three-volume series, *El servicio personal de los indios en el Perú (extractos de los siglos XVI, XVII, y XVIII y comienzos del XIX)* (1978–80), similar to the one here under review.

Zavala's pioneering work on Mexican, Guatemalan, and Peruvian colonial labor history has consisted largely in opening up little-known documentary sources and topics. This volume is a worthy continuation of that enterprise.

MURDO J. MACLEOD
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SILVIO ZAVALA. *El servicio personal de los indios en la Nueva España*. Volume 2, *1550–1575*. (Centro de Estudios Históricos.) Pedregal de Sta. Teresa, Mexico: El Colegio de México and El Colegio Nacional. 1985. Pp. 662.

This volume, like the first—as well as Silvio Zavala's *Asientos de la gobernación de la Nueva España* (1982) and *El trabajo indígena en los libros de gobierno del Virrey Luis de Velasco, 1550–1552* (1982)—is divided into eleven parts: "Evolución general," "Agricultura y ganadería," "Transportes," "Minería," "Servicios urbanos," "Visitas a provincias foráneas," "Marquesado del Valle," "Magistrados, otros funcionarios y pretendientes," "La Iglesia," "Obras públicas," and "Caciques, principales y comunidades indígenas." It covers the period of the viceroyalties of Don Luis de Velasco (the elder) and Don Martín Enríquez. It shares much, as well, with Zavala's three-volume *El servicio personal de los indios en el Perú* (1978–80).

What all of these publications have in common is a nether world between primary and secondary sources. They are, in essence, extensive summaries of documents, arranged chronologically and by category—the kind of notes one prepares while working in an archive. As such, they may serve the reader as initial passes at documents to be consulted in full at some later date or as substitutes for those documents should direct consultation be impossible (given the variety of published and archival sources employed, this may at times be necessary).

The second volume (there will be a third) examines the institution of draft Indian labor—the *repartimiento*—in the Viceroyalty of New Spain from its foundation in 1550 to 1575. Draft Indian labor was designed to render Indian slavery unnecessary and to counter the difficulties, primarily for the crown rather than for Indians, presented by the *encomienda*. Its introduction generated more problems than it solved, from the legal questions regarding its compatibility with the Indians' status as free vassals to the competition for Indian labor among *encomenderos* and a host of other would-be employers of Indians. Although the subject holds the promise of serving as an entryway to the nuts and bolts of Spanish-American colonization, that promise unfortunately falls victim to a plodding rendition of document after document after document. Thus, only those undertaking extensive research of their own will find this series valuable.

JEFFREY A. COLE
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MAGNUS MÖRNER. *Adventurers and Proletarians: The Story of Migrants in Latin America*. Assisted by HAROLD SIMS. Pittsburgh: University of Pittsburgh Press or UNESCO, Paris. 1985. Pp. xvii, 178. $19.95.

This little book by Magnus Mörner is a short but comprehensive survey of the present state of knowledge on migration to and from Latin America. Beginning with the colonial period, Mörner covers the history of foreign immigration into Latin Amer-

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ica up to and including the post–World War II era, emigrations out of the region (mostly to the United States), and the rural-urban migrations within it. In undertaking to treat of so vast a subject, Mörner does not content himself with just elucidating statistics—important and interesting as these may be—but also considers the matter of migrants' motivations and choices of destination, their integration and assimilation, their impact on the recipient society, and the causes of the more recent rural-urban and out-migration movements. And all of this is compacted into only one hundred thirty pages of text. In keeping his text short, Mörner to some degree sacrifices full discussion to rapid review, particularly in the four chapters (out of eight) that deal with the psychologically and sociologically more involved matters of motivation, assimilation, and impact—in which the really fascinating ramifications of human migration (as opposed to mere numbers) might have been more intimately explored.

Nevertheless, as a comprehensive treatment of the subject, the book is first-rate and much needed, considering the great amount of research and writing on all aspects of the subject over the past twenty years. Mörner has ably assembled and reviewed this mass of scholarship, notwithstanding its having appeared in a variety of languages, including Swedish, Polish, and Hungarian. For such an effort alone the book would be invaluable, but Mörner has also brought a judicious and discerning appraisal to his reportage that has filtered out any political, national, or ethnic biases in his sources. A lapse in this regard is his remark that the United States has "a preference for refugees from communist states" (p. 106); why communist states should produce refugees is left unsaid. In his final remarks Mörner convincingly demonstrates that the two current rival theories of migration—that is, the conventional Smithian approach, which stresses free choice and migration from high to low labor density, and the Marxist interpretation, which stresses dependency and the transportation of labor to the most profitable and exploitative enterprises—are both oversimplifications and absolutely untenable in many instances. But he prudently holds back from propounding any general theory of migration.

Mörner's frequent comparisons between different aspects of the immigrants' experience in Latin America and those of their counterparts in the United States make up another positive feature of the book. This feature, of course, aims at a global perspective. But, to provide a truly global view of nineteenth- and twentieth-century migration, contrasts, comparisons, and juxtapositions must be made between all the three groups of countries that received immigrants—namely, the United States, Latin America, and the British Dominions—a task that neither Mörner nor anyone else has yet attempted.

I must mention one final mechanical point: the footnotes are marred by an annoying number of single name and date references to books either that are not listed in the bibliography or that are listed under a different form of the name; thus, *Nogueira* is under *Rocha Nogueira* and *Bastos de Avila* under *Avila*. Several misspellings also occur, which is unfortunate because this book and its bibliography are now an unavoidable starting point for anyone interested in reading or doing research on Latin American immigration.

GEORGE F. W. YOUNG
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HARRIS GAYLORD WARREN. *Rebirth of the Paraguayan Republic: The First Colorado Era, 1878–1904*. Assisted by KATHERINE F. WARREN. (Pitt Latin America.) Pittsburgh: University of Pittsburgh Press. 1985. Pp. xvi, 379. $31.95.

No Latin American nation of the nineteenth century underwent a more profound structural and institutional transformation than obscure, isolated Paraguay. There, in the half-century before 1865, a remarkable system of authoritarian state "socialism" achieved impressive levels of national power and autarky, only to be destroyed in the devastating War of the Triple Alliance (1865–70) and replaced by a new system of laissez-faire liberalism that ushered in an era of impoverishment and dependency. Harris Gaylord Warren's new book, an extension of his *Paraguay and the Triple Alliance: The Postwar Decade, 1869–1878* (1978), is a detailed, descriptive survey of Paraguay during the years when a new liberal order of capitalist democracy was being consolidated in the country under the guidance of a nascent Colorado party.

Slogging chronologically through the seven successive Colorado administrations that dominated Paraguay's government between 1878 and 1904, Warren paints a portrait of the Colorados that is generally unflattering and occasionally sordid. Consistently he finds the party's leaders motivated not by ideological or philosophical conviction but by political and economic opportunism. Lacking any tradition of peaceful political compromise, they manipulated Paraguay's new democratic processes cynically and ruthlessly, using military force, political violence, intimidation of opponents, and electoral fraud to monopolize the spoils of office. Inheriting a bankrupt treasury and an economy ravaged by war, drought, flood, pestilence, and locusts, they soon turned to the one easy revenue-generating mecha-

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nism available to them: sale of state-owned assets, particularly land from the massive public domain accumulated by the prewar authoritarian state. Warren estimates that of the 16,329 square leagues of land in the Paraguayan public domain in 1870 all but 304 leagues had been transferred to private ownership—largely to land speculators and foreign extractive corporations—"for approximately $10 million in cash and promissory notes" by the end of the century (p. 171). A significant share of the proceeds apparently ended up in the pockets of Colorado officials.

Warren's assessment of Colorado economic management is ambivalent: lacking resources, skill, vision, and often personal integrity, Colorado leaders nevertheless created an economic environment—via laissez-faire capitalism and land sales to private, largely foreign, enterprises—that stimulated production and placed the nation on the road to economic recovery, albeit one dominated by foreign capital. The book's chapters on economic affairs bring together a wealth of valuable information on trade (controlled by Argentina), foreign investment (dominated by English capital), wages (forty-five cents per day for urban laborers in 1901; fifteen cents per day for rural workers), agriculture, industry, government finance, and banking.

Less satisfying is the book's treatment of social history. Despite tantalizing references to "latifundistas," "middle and commercial classes," "small farmers," "peons," and "squatters," the social structure and class configurations of postwar Paraguay remain unreconstructed. The reader waits in vain for any systematic analysis of the socioeconomic backgrounds of Colorado party leaders or of the social composition of the party's grass-roots constituency. The presumably profound impact of massive land sales on rural Paraguayan society receives only cursory discussion. Nor does the book address the historiographical assertion that Colorado politicians were rural landowners whereas their Liberal party opponents represented an emergent urban bourgeoisie.

It is no easy task, of course, to reconstruct the social history of a nation for which historical statistics are either nonexistent or unreliable and where large quantities of archival records have been scattered, lost, destroyed, or stolen. Warren's book enriches our understanding of Paraguay's late nineteenth-century political economy. Its gaps point the direction for future research.

MICHAEL GROW
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HERBERT BRAUN. *The Assassination of Gaitán: Public Life and Urban Violence in Colombia*. Madison: University of Wisconsin Press. 1985. Pp. xiii, 282. $32.50.

Herbert Braun has written an excellent study, and by far the most thorough we have in English, of the urban riot that took place on April 9, 1948, in Bogotá, Colombia and that has come to be known as the *bogotazo*. The book is also unusually well written; in particular, the chapters on the riot itself veritably sweep one along.

Nonetheless, this book is a good deal more than the study of a spontaneous riot of the *pueblo* of Bogotá. Similarly, it is much more (and less) than a definitive biography of Jorge Eliécer Gaitán, the popular *caudillo* whose assassination provoked the *bogotazo*, although several chapters encompass his early career and rise to prominence. Rather, the book is centrally an attempt to portray the impact of fundamental changes, such as urbanization and the expansion of a market economy, on the *convivialista* political culture of Colombia's political elites, a political culture in which, despite a history of deep-seated antagonism between Liberals and Conservatives, the leaders of the parties conducted politics in the manner of "conversations among gentlemen." Formally democratic, the system paid little heed to the voices of the *pueblo*, which the political leaders purportedly represented. Gaitán sought to challenge that order of things and, through his voice and actions, to bring about the effective entry of the *pueblo* into the political arena.

In certain respects Braun's book makes for unconventional history. Thus, although the organization is chronological and the form for the most part narrative, the result (and, I think the objective) is less a standard account of events than an evocation of the spirit of an age in Colombian politics (1930–50) and of the essence of the event (the *bogotazo*) that was its shattering culmination. The book is also not meant as a standard biography of Gaitán; for that one should consult Richard E. Sharpless's, *Gaitán of Colombia* (1978). Thus, one will not find here any detailed analysis of Gaitan's electoral strength, for example, and little concerning his personal life. One does emerge, nonetheless, with a real sense of the nature of Gaitán's leadership, of his challenge to the politics of elitist civility, and of what moved the crowd to fury on April 9, 1948.

The author's sources are oral as well as written. Thus, in addition to imaginative use of archival sources (including those of both the Colombian and the American Red Cross), he has interviewed many participants in the *bogotazo* itself and relied extensively on their impressions of that event.

For Braun, Gaitán is neither a socialist nor a revolutionary (both of which judgments are in accord with my evaluation) nor even, in the main, a

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populist. Quintessentially he is representative of a petty bourgeoisie seeking access to a largely closed social and political order. This appears an accurate enough assessment but, it seems to me, still ranks Gaitán with most Latin American leaders usually classed as populist.

In short, Braun, himself a Colombian by birth, has written a first-class study that succeeds in evoking the essence of an era, an event, and a man central to modern Colombian history.

ROBERT H. DIX
Rice University

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

HORST WALTER BLANKE and JÖRG RÜSSEN, editors. *Von der Aufklärung zum Historismus: Zum Strukturwandel des historischen Denkens.* (Historisch-Politische Diskurse, number 1.) Paderborn: Ferdinand Schöningh. 1984. Pp. 324.

JÖRN RÜSSEN, Von der Aufklärung zum Historismus: Idealtypische Perspektiven eines Strukturwandels. WOLFGANG WEBER, Wissenschaftssoziologische Aspekte des Strukturwandels der Geschichtswissenschaft von der Aufklärung zum Historismus. HILMAR KALLWEIT, Zur Topographie der Historie in enzyklopädischen Ordnungen des Wissens. HANS-JÜRGEN PANDEL, Pragmatisches historistisch Erzählen bei Kant: Zur Rehabilitierung einer historistisch missverstandenen Kategorie. HORST WALTER BLANKE, Aufklärungshistorie, Historismus und historische Kritik: Eine Skizze. HANS-JÜRGEN LÜSEBRINK, Aufklärerisches Erkenntnispotential versus institutionelle Erkenntnisschranken: Zur Geschichtsschreibung Henri Grégoires (1751–1831). GANGOLF HÜBINGER, Forschendes Erklären: Georg Gottfried Gervinus' Mittelstellung zwischen Aufklärung und Historismus. DIRK FLEISCHER, Der Strukturwandel der Kirchengeschichtsschreibung in der Aufklärung. WOLFGANG ERNST, J. J. Winckelmann im Vor(be)griff des Historismus. HANS-JÜRGEN PANDEL, Die bloss empirischen und die philosophischen Historiker: K. H. L. Pölitz zwischen Aufklärung und Historismus. HORST WALTER BLANKE, Ludwig Wachlers Historiographiegeschichte im Umbruch zwischen Aufklärung und Historismus. HORST WALTER BLANKE, Diskursgrenzen des Historismus: Die wissenschaftshistorischen Schriften Lamprechts und Breysigs und erneuter Paradigmenwechsel der Historie. JÖRN RÜSSEN, Ende des Historismus? Chancen historischer Vernunft.

NORBERT BOLZ and BERND WITTE, editors. *Passagen: Walter Benjamins Urgeschichte des neunzehnten Jahrhunderts.* Munich: Wilhelm Fink. 1984. Pp. 197.

BERND WITTE, Paris—Berlin—Paris: Zum Zusammenhang von individueller, literarischer und gesellschaftlicher Erfahrung in Walter Benjamins Spätwerk. BURKHARDT LINDNER, Das "Passagen-Werk", die "Berliner Kindheit" und die Archäologie des "Jüngstvergangenen." RAIMAR STEFAN ZONS, Annäherung an die "Passagen." IRVING WOHLFARTH, Et cetera? Der Historiker als Lumpensammler. SUSAN BUCK-MORSS, Der Flaneur, der Sandwichman und die Hure: Dialektische Bilder und die Politik des Müssiggangs. PHILIPPE IVERNEL, Paris, Hauptstadt der Volksfront oder das postume Leben des 19. Jahrhunderts. NORBERT BOLZ, Bedingungen der Möglichkeit historischer Erfahrung. H. D. KITTSTEINER, Walter Benjamins Historismus.

LESTER GRINSPOON, editor. *The Long Darkness: Psychological and Moral Perspectives on Nuclear Winter.* (Yale Fastback, number 31.) New Haven: Yale University Press. 1986. Pp. vii, 213. Cloth $25.00, paper $7.95.

LESTER GRINSPOON, Introduction. CARL SAGAN, Nuclear War and Climatic Catastrophe: Some Policy Implications. ERIK H. ERIKSON, Evolutionary and Developmental Considerations. STEPHEN JAY GOULD, A Biological Comment on Erikson's Notion of Pseudospeciation. ROBERT J. LIFTON, Imagining the Real: Beyond the Nuclear "End." JOHN E. MACK, National Security Reconsidered. HENRY STEELE COMMAGER, Unexamined Assumptions and Inescapable Consequences. J. BRYAN HEHIR, Opposing the Nuclear Threat: The Convergence of Moral Analysis and Empirical Data. JEROME D. FRANK, Afterword: Nuclear Winter and the Will to Power.

LEWIS H. LAPHAM, editor. *High Technology and Human Freedom.* (Smithsonian International Symposia Series.) Washington, D.C.: Smithsonian Institution. 1986. Pp. 170. Cloth $19.95, paper $9.95.

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JACQUES LE GOFF and PIERRE NORA, editors. *Constructing the Past: Essays in Historical Methodology*. Foreword by COLIN LUCAS. New York: Cambridge University Press or Editions de la Maison des Sciences de l'Homme, Paris. 1985. Pp. v, 217. $39.50.

COLIN LUCAS, Introduction. FRANÇOIS FURET *translated by David Denby*, Quantitative Methods in History. PIERRE CHAUNU *translated by Martin Thom*, Economic History: Past Achievements and Future Prospects. PIERRE VILAR *translated by Ian Patterson*, Constructing Marxist History. EMMANUEL LE ROY LADURIE *translated by David Denby*, The History of Climate. ANDRÉ BURGUIÈRE *translated by David Denby*, Demography. ALPHONSE DUPRONT *translated by Martin Thom*, Religion and Religious Anthropology. GEORGES DUBY *translated by David Denby*, Ideologies in Social History. JACQUES LE GOFF *translated by David Denby*, Mentalities: A History of Ambiguities. MONA OZOUF *translated by David Denby*, The Festival in the French Revolution. ROGER CHARTIER and DANIEL ROCHE *translated by David Denby*, New Approaches to the History of the Book.

JACQUES JULLIARD and SHLOMO SAND, editors. *Georges Sorel en son temps*. Paris: Seuil. 1985. Pp. 474. 165 fr.

JACQUES JULLIARD, Actualité de Georges Sorel. MADELEINE REBÉRIOUX, La place de Georges Sorel dans le socialisme au tournant du siècle. PETER SCHÖTTLER, "La Commune ouvrière en formation"? Georges Sorel et les bourses du travail. ZEEV STERNHELL, Georges Sorel: Le syndicalisme révolutionaire et la droite radicale au début du siècle. MARIA MALATESTA, Georges Sorel devant la guerre et le bolchevisme. ROBERTO VIVARELLI, Georges Sorel et le fascisme. GEORGES GORIELY, Georges Sorel et la révolution. PIERRE ANDREU, La préparation morale à l'absolu marxiste. LARRY PORTIS, La cinématique marxiste de Georges Sorel. DANIEL LINDENBERG, Mouvement prolétarien et révolution religieuse: Georges Sorel critique de Renan. THIERRY PAQUOT, Les écrits économiques de Georges Sorel. SHLOMO SAND, Lutte de classes et conscience juridique dans la pensée de Georges Sorel. SERGIO ROMANO, Georges Sorel et Benedetto Croce. PETER NIJHOFF, Georges Sorel et Émile Durkheim: Convergences et divergences, 1894–1899. JEAN-LUC POUTHIER, Georges Sorel et Roberto Michels. BRUNO SOMALVICO, Georges Sorel et Guiseppe Prezzolini: Convergence ou malentendu? 1908–1911. GIOVANNI BUSINO, Georges Sorel et Vilfredo Pareto. ROBERT PARIS, Géographie du sorélisme. JUTTA SCHERRER, Georges Sorel en Russi: Le cas des bolcheviks de gauche. LARRY PORTIS, Georges Sorel dans les pays anglo-saxons. MICHEL PRAT, Georges Sorel en Allemagne.

MAURICE VAÏSSE, editor. *8 Mai 1945: La victoire en Europe*. (Actes du Colloque International de Reims, 1985; Collection L'Histoire Partagee.) Lyon: Manufacture. 1985. Pp. 458. 120 fr.

MAURICE VAÏSSE, Avant-propos. JEAN-BABTISTE DUROSELLE, Introduction générale. LEE KENNETT, La stratégie anglo-américaine en 1945. MARC FERRO, La stratégie soviétique en 1945. ANDRÉ MARTEL, La stratégie française en 1945. MARLIS G. STEINERT, L'attitude allemande face à capitulation inconditionnelle. BRUNO ARCIDIACONO, Le précédent italien: Les armistices de 1943; La dimension interalliée. GEORG KREIS, La fin de la guerre en Italie du Nord en 1945. LORD DACRE, La fin à Berlin. MAURICE VAÏSSE, Remarques sur la capitulation à Reims le 7 mai 1945. WILLIAM R. KEYLOR, Des buts de guerre aux buts de paix: La politique "européenne" des Etats-Unis de Yalta à Potsdam. JEAN LALOY, La politique de l'U.R.S.S. à l'issue de la guerre principalement en Europe. RENÉ GIRAULT, La France est-elle une grande puissance en 1945? RAYMOND POIDEVIN, La politique allemande de la France en 1945. DAVID DILKS, La Grande-Bretagne et le monde extérieur en 1945. CHARLES-LOUIS FOULON, Le général de Gaulle et la France face à la victoire. ANTHONY ADAMTHWAITE, L'opinion publique britannique face à la victoire de 1945: Mythe et réalité. KLAUS-JÜRGEN MÜLLER, L'Allemagne face à la défaite: Conceptions politiques alternatives. JEAN-PIERRE RIOUX, "Cette immense joie pleine de larmes": Les Français et le "jour V." ANNIE REY-GOLDZEIGUER, Le 8 mai 1945 au Maghreb. JEAN-PIERRE HUSSON, Le retentissement de la victoire dans la Marne. GEORGES CLAUSE, Reims autour du 7 mai 1945. FRANÇOIS COCHET, Aspects et enjeux du retour des prisonniers et déportés dans la Marne. FRANÇOIS BÉDARIDA, Conclusions.

PHILIPPE ARIÈS and ANDRÉ BÉJIN, editors. *Western Sexuality: Practice and Precept in Past and Present Times*. (Family, Sexuality, and Social Relations in Past Times.) New York: Basil Blackwell. 1985. Pp. ix, 220. $24.95.

PETER LASLETT, Foreword. PHILIPPE ARIÈS and ANDRÉ BÉJIN, Preface. ROBIN FOX, The Conditions of Sexual Evolution. MICHEL FOUCAULT, The Battle for Chastity. PAUL VEYNE, Homosexuality in Ancient Rome. PHILIPPE ARIÈS, St. Paul and the Flesh. MICHAEL POLLAK, Male Homosexuality—or Happiness in the Ghetto. PHILIPPE ARIÈS, Thoughts on the History of Homosexuality. JACQUES ROSSIAUD, Prostitution, Sex, and Society in French Towns in the Fifteenth Century. ACHILLO OLIVIERI, Eroticism and Social Groups in Sixteenth-Century Venice: The Courtesan. ANGELINE GOREAU, Two English Women in the Seventeenth Century: Notes for an Anatomy of Feminine Desire. JEAN-LOUIS FLANDRIN, Sex in Married Life in the Early Middle Ages: The Church's Teaching and Behavioural Reality. PHILIPPE ARIÈS, Love in Married Life. PHILIPPE ARIÈS, The Indissoluble Marriage. ANDRÉ BÉJIN, The Extra-Marital Union Today. HUBERT LAFONT, Changing Sexual Behaviour in French

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A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

REVIEWS OF BOOKS

TO THE EDITOR:

I was most pleased to discover that a review of my recently published book, *The American Catholic Experience*, appeared in the journal so soon after its publication in October of 1985 (*AHR*, 91 [1986]: 726). I must admit, however, that I was not very pleased with the lukewarm review. Fortunately most of the reviews of my book have not been so lukewarm so I was quite able to recover from my disappointment. But there was one statement made by the reviewer that I could not dismiss so readily. It was his unfair and unfounded statement that "Dolan obviously has little use for the immigrant period, which he sees as a mostly backward anomaly that delayed unnecessarily U.S. Catholicism's inevitable ascension from Plato's cave of ignorance into the light of the modern world." In that statement he is not only impugning my reputation as an historian of immigration and religion, but is making a judgment that has no basis in fact in the book under review.

I repudiate his statement and would like it known for the public record that as an historian I have a special love and fascination for the immigrant period of American history and have spent the last twenty years of my life studying and writing about this aspect of American history. I take the religion of the immigrants seriously and nowhere in any of the three books that I have written on U. S. Catholicism in the immigrant period do I characterize this era of Roman Catholicism or these people as a "backward anomaly." It was a great period in our nation's history and these people hardly dwelled in what the author describes as "Plato's cave of ignorance." No historian with any degree of competence or cultural sensitivity could seriously endorse such a characterization. I surely do not.

The irony of the reviewer's judgment is that in a book that covers almost four hundred years of history fifty percent of the text is given over to a study of the immigrant church in the period from 1820 to 1920. If I had so little use for this period of history, why would I dedicate half of the book to it? In my opinion this was the heart of the book where I did much of my most creative work. It is regrettable that the reviewer missed this entirely and dismissed that part of the book in a flippant and inaccurate manner.

JAY P. DOLAN
University of Notre Dame

PROFESSOR JOSEPH A. VARACALLI REPLIES:

I can only offer Professor Dolan a "lukewarm" apology for my review of his book *The American Catholic Experience*. I apologize sincerely for the sarcastic sentence in question; it was needless and carried with it a certain exaggeration. I do not, however, retract the intellectual charge that was contained within the sentence, to wit: that a liberal or "Americanizer" bias colors his understanding of the immigrant period from 1820–1960 (or, for that matter, of the total American Catholic experience). I argued, and I stand by this, that his work displayed an "evolutionary" thinking that suggests that the thoughts, actions, and stance to the world of the immigrant Church were inferior to those of the "republican interlude" (1780–1820) and that the latter period was, in turn, vindicated by Dolan's own very selective understanding of the significance of Vatican II for contemporary U.S. Catholicism.

I have neither impugned Professor Dolan's reputation as an historian nor argued that his book was not important and could not intellectually withstand a good deal of criticism. Consistent with my training

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as a "sociologist of knowledge," I believe that all interpretation is colored by a set of values that influences a work, and, for the sake of objectivity, these values must be brought to the fore of the discussion. The intrusion of values, while inevitable, may be either subtle or blatant; I argued that in Dolan's case the intrusion is subtle but nonetheless very real.

I am sorry for Professor Dolan that my review was not more positive. The simple fact remains that I am not part of the liberal Catholic intellectual establishment in the U.S. that has had its way, more or less, for the past two decades and that would "naturally" welcome Dolan's analysis for both theoretical and material reasons. The relatively more traditional, pro-ecclesiastical values that influence my own interpretation of events has led me to be less sanguine about the contemporary state of affairs of American Catholicism and, by implication, relatively more appreciative of the religiosity of the immigrant Church than would the contemporary liberal Catholicism of Dolan's which seemingly sees as its normative model that of liberal Protestantism. My position, by the way, does not imply that I hold the immigrant period as an ideal. My own understanding is pluralistic and decidely non-evolutionary; it is that of the Papal historian von Ranke for whom "each age is immediate unto God." Put negatively, every "gain" in the post-Vatican II Church incurs its equal cost; the present age is, on balance, no closer to a "correct" understanding of the relationship of the individual and Church to God than was the immigrant period.

Professor Dolan's argument that the amount of space and time he spent studying the immigrant period is proof of his respect and love for the immigrant Church is simply not logical. I presume that the Marxist who studies capitalism and the ethicist who studies Nazism are not respectful and in love with their respective subjects. I am not suggesting that Dolan's oppositional stance is as obvious as that painted immediately above; it is more akin to the "progressive" anthropologist who patronizingly "loves" the natives he or she studies not for what they are but for what they can become under the "right" (in this case, modern American and modern theological) environmental influences.

I encourage scholars to read Professor Dolan's important book both to learn much about the American Catholic Church that is valuable and also to decide for themselves the degree of truthfulness in my charge. Let me end by stating the conviction that the present-day liberal monopoly in Catholic scholarship will increasingly be challenged by the emerging neo-orthodox school of which I am a member. The resulting engagements, disagreements, compromises, and, perhaps, emerging consensus produced by such an intellectual environment of competing biases can naught but push forward the search for truth to the benefit of both Church and academy.

JOSEPH A. VARACALLI
Nassau Community College

TO THE EDITOR:

Patricia Crone's review of my *Islamic Conception of Justice* (*AHR*, 91 [1986]: 160), consisting of errors and distortion of my ideas and interpretations, compels me to reply.

The first two paragraphs contain statements that the book consists of "some two hundred forty pages"—as if the quality of a book, especially dealing with theories, is measured by quantity—and its author "has nothing of substance, let alone anything new to say on the subject." But if nothing is new, where else are my ideas and interpretations about justice to be found? Crone also makes the statement: "Contrary to what one might expect, the book is not devoted specifically to Islamic views on legal justice but rather to Islamic views in . . . legal, social, political, theological, philosophical, and ethical senses." Justice means to her legal justice only, but other aspects are "wholly irrelevant to justice." Bearing in mind that in Islam, law and state are inseparable from religion, a discussion of law without an understanding of religion and politics is misleading and meaningless. As I pointed out in the first chapter, the debate on justice first began on the political level before it moved to higher levels of sophistication, and a discussion of legal justice without relevance to other aspects would be incomprehensible. Thus, I devoted a chapter to each aspect to provide a balanced theory of Islamic justice. From my discussion of diverse views, a general theory emerges as the debate on justice moved from one level to another, and an overall theory is provided. In my discussion on legal justice, the sources and schools of law are dealt with in the first chapter. In the central consideration, however, of how much (in quantity and quality) the law contains of the elements of justice and how justice is ultimately realized (or not realized), she seems to have no idea what jurisprudential justice means.

The third paragraph deals with *minutiae*. She claims to have found some half a dozen errors. Two are related to the use of "Sunni" and "Sunnism." She maintains that "Sunnism was born not at the time of the Prophet, but in the ninth century and later." But these terms were not always used alike. Sunnism is a theological doctrine born in the Abbasid period, but "Sunni" was applied to followers of the orthodox community in contrast to followers of heterodox groups (especially Shi'a) who came into existence long before Abbasid times. Crone maintains that

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"jabr" (predestination, involuntarism) was not the official doctrine of the Umayyad regime. If it were not official, on what grounds were the Qadarites, opponents to "jabr," persecuted and some put to death specifically on charges of voluntarism? Moreover, Mu'awiya, founder of the Umayyad dynasty, and his successors (one of whom sent an official letter to provincial governors) asserted that their rule was predicated by God (pp. 26–27). Crone also claims that the Murji'ites did not support the Umayyad regime. I pointed out that they did indirectly by calling for the postponement of judgment on all matters of political justice until Resurrection, because their message, though addressed to all believers, was well-known to have been against Kharijites and other critics of the regime. Crone also stated that Shafi'i' ijma' had no bearing on the choice of sovereign. I made reference to ijma' as the doctrine of *vox populi vox Dei* on the basis of the Prophet's Tradition, not Shafi'i's ijma'. In the footnote (p.18, n.12), I referred to that Tradition in my "introduction" to Shafi'i's *Islamic Jurisprudence*, not to Shafi'i's text.

In the final paragraph, Crone deals with the presumed "purpose" and "message" of my book. She said: "the general message" is that "the Islamic conception of justice was admirable," and the work's "'purpose' is to evoke admiration for Islam." But, she adds, I failed in my purpose. She put into the mouth of the author what he did not say. No other Western scholar who reviewed my works, including the book under review, has ever imputed such a charge. In fact, my book on justice contains criticism of the administration of justice (Chap. 7) and other matters (final chapter and appendix). Thus her allegations are unfounded, and the tone of her review is pretentious and discourteous.

MAJID KHADDURI
The Johns Hopkins University

Patricia Crone was invited to reply to Majid Khadduri's letter and declined to do so.

THE EDITOR

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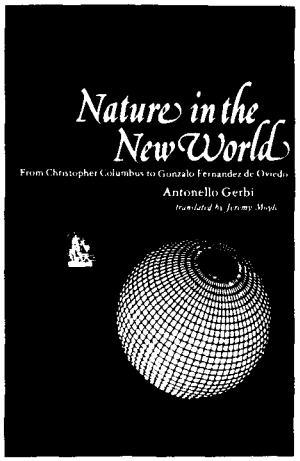
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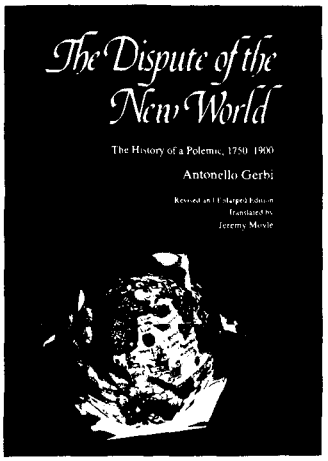
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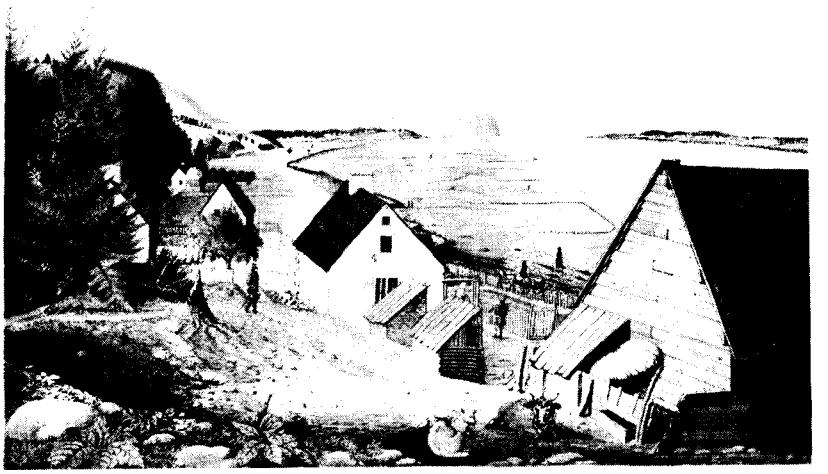
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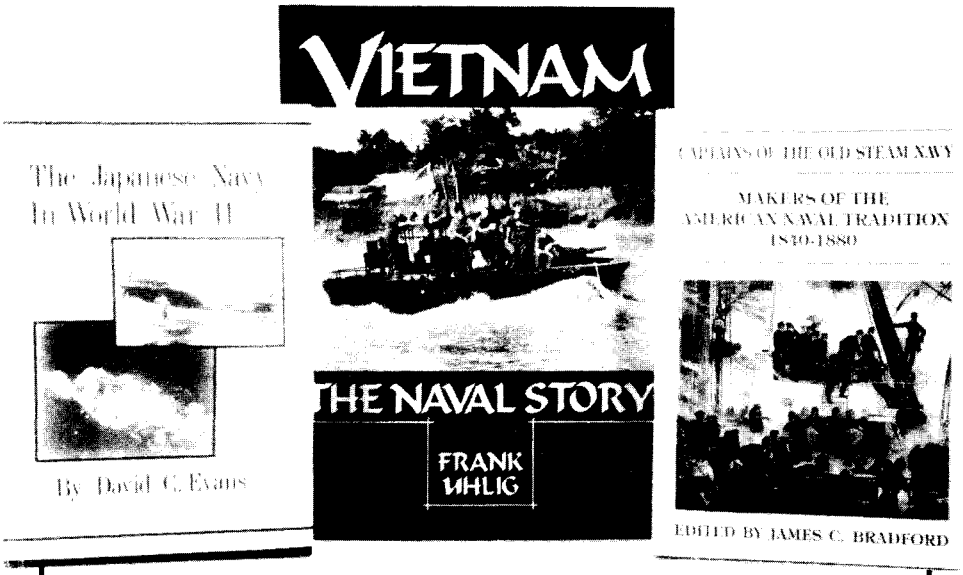
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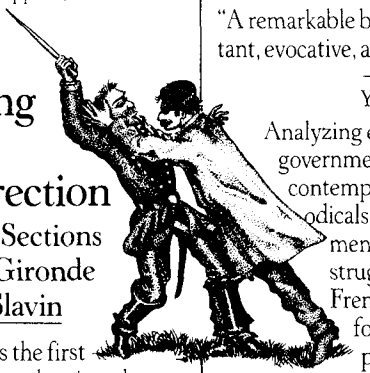
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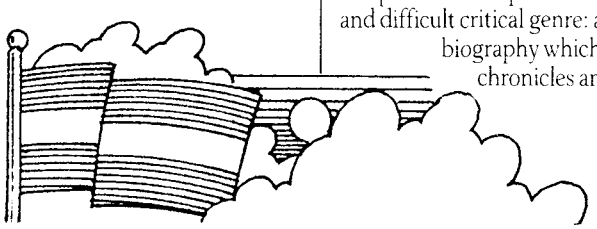
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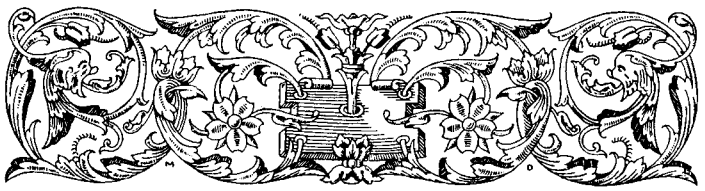


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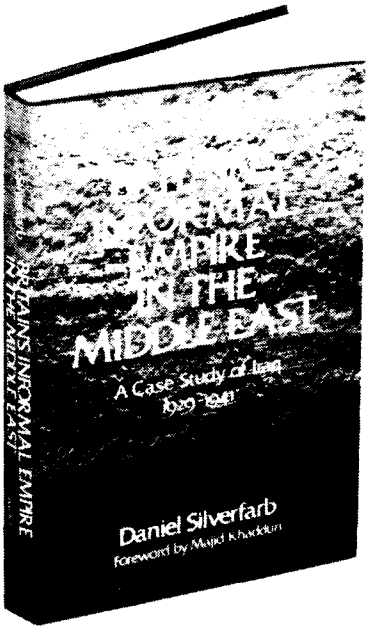
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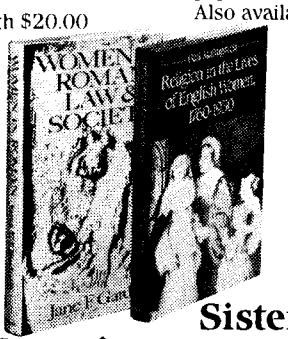
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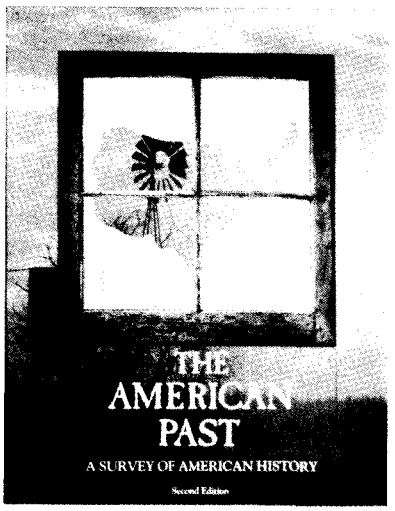
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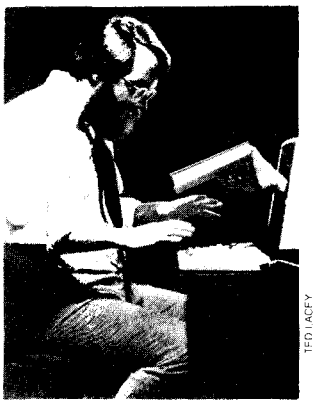
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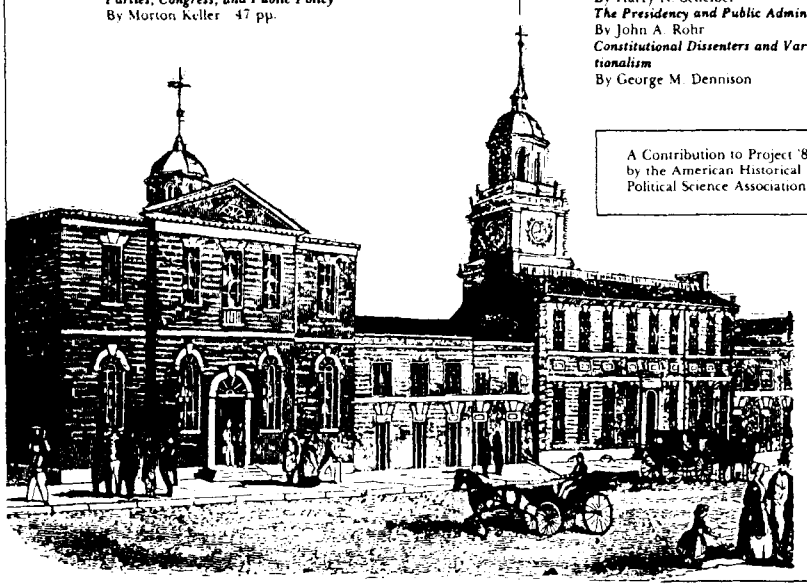
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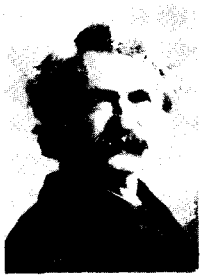
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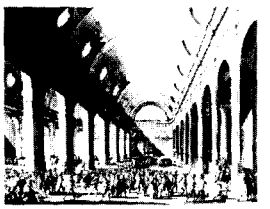
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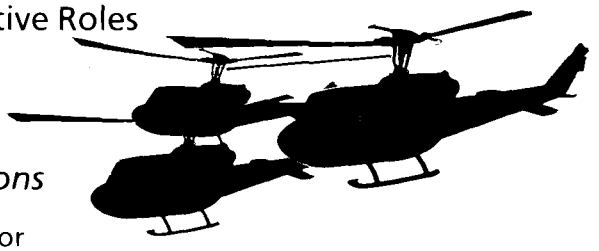
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